DISSERTATIONS

RELATIVE TO THE

NATURAL HISTORY

OF

ANIMALS AND VEGETABLES.

Tranſlated from the Italian of the

ABBÉ SPALLANZANI,

Royal Profeſſor of Natural Hiſtory in the Univerſity of PAVIA, Superintendant of the PUBLIC MUSEUM, and FELLOW of various learned SOCIETIES.

To which are added,

TWO LETTERS from Mr. BONNET to the AUTHOR.

And (to each Volume of this TRANSLATION) an APPENDIX, the firſt containing a Paper written by Mr. HUNTER, F.R.S. and the Experiments of Dr. STEVENS on Digeſtion; the ſecond a Tranſlation of a Memoir of Mr. DEMOURS, and Mr. DEBRAW's Paper on the Fecundation of Bees.

VOL. II.

LONDON:

PRINTED FOR J. MURRAY, N° 32, FLEET-STREET.
M DCC LXXXIV.

INTRODUCTION.

IN the firft and fecond Differtations of this volume, I acquit in part the promife I had long fince made in my *Profpectus concerning Animal Re-productions*. In that piece I gave intimation of my difcovery of the pre-exiftence of the germ in one fpecies of frog. This difcovery is related at length in the firft differtation. That fo long a fpace of time has elapfed fince the obfervation was made, I confider as a very fortunate circumftance; for, having examined other animals, and having found that the fame thing is true of them, I have ftill ftronger reafon for prefuming, that the exiftence of the germ in the female before fecundation, is one of the moft general laws of nature. While we are in queft of one truth, it generally happens that others offer themfelves as it were fpontaneoufly. Accordingly I have been led by the obfervations, which fhew the pre-exiftence of the germ, to difcover that an order of animals, confidered by naturalifts as oviparous, is in reality viviparous.

The fubject of my fecond differtation is the artificial fecundation of various animals, of which the firft lines may be feen in the fame profpectus. This was accomplifhed by means of femen taken from them; and thus I have fucceeded as well, as if the male himfelf had performed his function.

The clofe analogy between animals and vegetables, induced me to enquire, whether the embryos of plants exift in like manner in the ovarium before fecundation. From the third differtation it will appear that this is really the cafe. Thefe enquiries have alfo convinced me, that the fecundating duft is not fo abfolutely neceffary as botanifts in general fuppofe.

The fame analogy ought, perhaps, to have led me to attempt the artificial fecundation of plants; for, though feveral have undertaken fuch experiments, and fucceeded in them, I know not whether any one has done this with truly philofophical views, and in order to difcover the manner of this wonderful operation. To confefs the truth, I would not have avoided this tafk, if engagements of another nature had not prevented me. I have, however, exhorted others to undertake it, and

at

at the ſame time have propoſed ſome views that may ſhew the way to ſucceſs.

It is ſaid by many, that fecundation is among the myſteries of nature; and, like many of her operations, an object of admiration, rather than of enquiry. Such an opinion is highly agreeable to the idleneſs of man. In times paſt, I acknowledge that generation, both in animals and plants, was involved in darkneſs, impenetrable to the human eye; but, ſince the appearance of Haller and Bonnet, this gloom has been rendered much leſs thick. I am very far from thinking that I have diſſipated it entirely, yet I would fain hope that, by my means, it has been ſomewhat cleared, and that a light, leſs feeble and uncertain, now ſhines through it. The following diſſertations will enable the learned reader to judge whether this opinion is well founded, or merely the offspring of ſelf-love.

DISSERTATION

CONCERNING THE

GENERATION

OF

CERTAIN ANIMALS.

CHAP. I.

GENERATION OF THE GREEN FROG.

I. THUS I chuſe to denominate the frog in queſtion, from the green hue of the upper part of its body, and from its living in water, particularly in puddles and ditches, whereas other ſpecies live on dry land and trees; but of theſe laſt I ſhall have occaſion to ſpeak afterwards.

II. The head of the male is furniſhed with two membranous veſicles, which ſwell very much when the animal croaks. The great toe of the fore foot has a fleſhy prominence, which becomes very perceptible at the ſeaſon of their amours: the female is deſtitute of theſe parts, and has the back and thighs marked with black ſpots, which are ſeldom viſible in the male.

III. This ſpecies is not to be confounded with that called by Roeſel *rana viridis aquatica.* A glance of his beautiful coloured figures, will be ſufficient to ſhew that they are totally diſtinct. His exceeds all other frogs in ſize; mine is the ſmalleſt of all, and is ſcarce $\frac{1}{3}$ ſo large as Roeſel's; beſides his has three yellow ſtripes on the back and ſides, which in mine are totally wanting (*a*).

IV. The amours begin in April, and end in May; they are, however, influenced by the temperature of the atmoſphere; during their continuance the males maintain an inceſſant croaking. In autumn and winter the immature eggs lie all in the ovarium, which is divided into two lobes; theſe lobes conſiſt of leſſer lobes, each of which is inveſted with a peculiar membrane. The eggs are of two ſizes; ſome very ſmall, ſo as to be ſcarce viſible by the naked eye; others ſeven or eight times larger; both kinds are globular. The ſmaller are of a livid grey colour; of the larger one hemiſphere is white, and the other black. The ſlighteſt touch is ſufficient to burſt them, after which they are reſolved into a cineritious viſcid liquor.

V. The ovarium of this, as well as many other ſpecies, appears to be externally covered with black points, which have been the ſource of a memorable miſtake; for Valliſneri (*b*), and other celebrated naturaliſts, have

(*a*) This eſſential difference was ſtill more apparent when I came to compare mine with ſome of Roeſel's, brought me by the fiſhermen.

(*b*) Opere fiſico-mediche. T. I. Oſſervazioni intorno le rane.

ſuppoſed

ſuppoſed them to be the rudiments of the tadpole. The error aroſe from their being contented with firſt appearances, and ſearching no further; for the ſpots lying cloſe to the eggs, might eaſily induce the obſerver to believe that they form part of them, and are therefore ſo many fetuſſes. But the matter is cleared up by the following experiments: 1. When the common membrane is removed, theſe points ſtill adhere to it. 2. And if the eggs are inſpected one by one, both externally and internally, no veſtige of this black ſpot can be perceived. If the membrane of the ovarium be examined by the miſcroſcope, it will then be ſeen that theſe points are black ſpots of an irregular ſhape. They are not peculiar to this membrane, for they are found in the area of the meſentery, and upon the heart.

VI. If the eggs be again examined in ſpring, we ſhall ſtill find them in the ovarium, but conſiderably enlarged; (thoſe I mean of the larger ſize, IV). and they will be found to be mature, when the male is coupled with the female. The copulation of this ſpecies is juſt like that of thoſe obſerved by Swammerdam and Roeſel (*a*). The male climbs upon the back of the female, and paſſing his fore legs under her axillæ, brings them to meet upon the breaſt, and there claſping his fingers, holds her cloſe, till ſhe has diſcharged all her eggs. The duration of this proceſs is inverſely proportional to the warmth of the atmoſphere. When this is conſiderable, the female will be free in five

(*a*) Biblia Naturæ. Hiſt. Nat. Ranar.

or ſix days; but in a cold ſeaſon, the embraces of the male continue for eight or nine days. This I learned by putting frogs in large veſſels full of water, when their amours were about to commence, and confining them till they had brought forth their eggs. And, although Valliſneri aſſerts, upon the authority of a ſingle experiment, that frogs do not bring forth in cloſe veſſels, it is certain that numberleſs facts prove the contrary, as I, and, before me, Swammerdam and Roeſel have found.

VII. It is true, as he aſſerts in another place, that the female does not diſcharge her eggs when ſhe is kept conſtantly ſeparate from the male. I ſay *conſtantly*; for if they be pulled aſunder when the eggs are deſcended into the cavity of the uterus, they are diſcharged, though the female is kept ſeparate, but they are not prolific.

VIII. If the ſituation of the eggs, at the time of copulation, be examined, during the firſt days they will be found in the ſac of the ovarium; and, during the ſucceeding, partly in the ovarium, and partly in the oviducts, and at laſt all in the uterus, except the ſmall ones, which remain attached to the ovarium. The eggs, when in the ovarium, are ſmaller than when in the oviducts and uterus. In theſe ſituations they are enveloped with that viſcid tranſparent mucilage, which is improperly called *frog's ſeed*.

IX. Of the various trials I have made, in order to aſcertain whether eggs taken from the ovarium, the oviducts, and the uterus, when the male is embracing the female, would

would be prolific, I muſt own that not one has ſucceeded. As this is a point of extreme importance, I repeated my experiments to ſatiety; and in my journals I find, that I have opened one hundred and fifty-ſix females while they were embraced by the male, of not one of which did the eggs ever bring forth young, though I immediately placed them in water; whereas thoſe that were excluded ſpontaneouſly by the female, were all prolific. I have even taken further pains. The diſcharge of eggs laſts about an hour; during this proceſs I killed a female, and put the eggs that remained in the body in the water into which thoſe diſcharged by the animal fell; but the latter produced tadpoles, while the former became an offenſive putrid maſs. From theſe facts I concluded, that the fecundation of the eggs does not take place within, but without the body; whence it appears how far Linnæus was miſtaken, when he pronounced, in his uſual deciſive tone, "Nullam in rerum natura, in ullo vivente corpore fieri fecundationem vel ovi impregnationem extra corpus matris (*a*)."

X. Hence we likewiſe ſee the falſehood of the ſtrange opinion of profeſſor Menzius, who ſuppoſed that, while the male embraces the female ſo cloſely, the ſeed is emitted from the fleſhy prominence of the toe, and paſſing through many windings unknown to us, penetrates into the thorax, and there impregnates the eggs (*b*).

(*a*) See Artedis Ichthyology, part ii. p. 32.

(*b*) Generatio παρα δοξος in rana conſpicua.

XI.

XI. But if in this ſpecies fecundation takes place out of the body of the female, ſhall we ſuppoſe that the male ejects ſemen upon the eggs as they are diſcharged (a)? This, with reſpect to other ſpecies, is the opinion of Swammerdam, and adopted by Roeſel; though he afterwards ſays, that he cannot determine whether the eggs in the uterus are impregnated by the ſemen out of the body. To clear up this point, I have paid unremitting attention to every phænomenon. As ſoon as the eggs begin to be diſcharged, the agitation of the female is extreme; ſhe darts backwards and forwards, riſes towards the ſurface of the water, and then ſinks, keeping the hind legs conſtantly ſtretched out, and croaking in a low voice. The male, that keeps his hind legs cloſe to his body, throws himſelf into ſtrange contortions, and accompanies the croaking of the female with a kind of interrupted noiſe, which I cannot expreſs by words. I moreover obſerved, that an obtuſe tumid point, which I ſuſpected to be the penis, was elongated, and now and then brought towards the eggs neareſt the vent; but I could not perceive any emiſſion from this ſuppoſed penis. The ſemen, however, might be in ſuch ſmall quantity, or ſo tranſparent, as not to be viſible.

XII. To determine the queſtion, I placed ſome couples, of which the female was beginning to diſcharge her eggs, in empty veſſels. As I knew that in theſe amphibious animals fecundation always takes place in

(a) In the frog, toad, newt, and ſeveral other animals, the eggs are diſcharged at the extremity of the alimentary canal.

water,

water, my hopes from this expedient were not very ſanguine; but fortune was more propitious than I could have expected. The male is ſo much attached to the female, that, notwithſtanding he is taken out of his natural element, he perſiſts in performing his office. I now ſaw that there ſpirted a ſmall jet of limpid liquor from the tumid point in the vicinity of the anus, upon the eggs hanging out at the vent of the female. This phænomenon took place only at intervals, the female from time to time diſcontinuing the diſcharge of eggs, and the male the emiſſion of ſemen. I was an eye-witneſs of this curious ſcene in ſeven couples of frogs ſet in the dry; it did not entirely ceaſe, till all the eggs were expelled from the uterus. The eggs being afterwards put into water, and bringing forth young, I heſitated not to ſuppoſe, that the liquor emitted by the male was real ſemen; as I afterwards found, by diſcovering it in the veſiculæ ſeminales, and that the point was the penis.

XIII. The abbe Nollet, alluding in a letter dated nine years ago, to the hint in my proſpectus, of my diſcovery of the exiſtence of the tadpole before fecundation, expreſſes himſelf in this manner: " What you ſay of the exiſtence of the tadpole, before we can perceive any act of fecundation, has particularly ſtruck me. About thirty years ago, Mr. Reaumur and myſelf made many reſearches relative to this ſubject. We attended for weeks very diligently and patiently, to what paſſed while the male embraced the female. I remember putting breeches of waxed taf-

fety

fety on the male, and watching a long time, without perceiving any appearance that denoted an act of fecundation."

I will not here stop to enquire by what fatality it happened, that two naturalists, so intelligent and attentive, failed in their enquiries. The idea of the breeches, however whimsical and ridiculous it may appear, did not displease me, and I resolved to put it in practice. The males, notwithstanding this incumbrance, seek the females with equal eagerness, and perform, as well as they can, the act of generation; but the event is such as may be expected: the eggs are never prolific, for want of having been bedewed with semen, which sometimes may be seen in the breeches in the form of drops. That these drops are real seed, appeared clearly from the artificial fecundation that was obtained by means of them (*a*).

XIV. Having discovered every thing relative to the situation and manner of impregnation, my next business was to observe the eggs, till the young animal should appear. The eggs lie in the centre of a white mucilage, which consists of transparent spherical masses connected together. Round the egg are two concentrical membranes, of which the innermost, when pierced with a needle, discharges a fluid as limpid as water. The egg is round, and has a smooth surface, of which one hemisphere is black, and the other white. This is seen at Fig. 1. Plate I. Nº III. shews fifteen eggs, with their mucilage; each lies in the midst of a circle,

(*a*) See Dissertation VIII.

made

made by theſe membranes. To ſee the other membrane clearly, the gluten muſt be magnified as at N° II. C is the external, B the internal membrane, and D the gluten.

XV. When the hot ſeaſon is far advanced, the obſerver ſoon perceives the lineaments of the tadpole. The egg grows for ſome hours without loſing its round ſhape, it is next elongated; the white hemiſphere becomes darker, and the black changes into a longitudinal furrow, terminated by two perpendicular proceſſes. And as it increaſes as well in bulk as length, the internal circular membrane is dilated, and contains more fluid. Compare N° I. fig. 2. with N° II. fig. 1.

By tracing thus the progreſs of the evolution, we come to perceive that theſe bodies are not eggs, as Naturaliſts ſuppoſe, but real tadpoles. The furrow and the proceſſes become longer; the ſuppoſed egg aſſumes a pointed figure; the whitiſh hemiſphere dilates, and the black is incurvated. The pointed part appears to be the tail of the tadpole, and the other the body. Further, the oppoſite end takes on the appearance of the head, in the fore part of which the form of the eyes is viſible, though they are yet cloſed. The two proceſſes alſo, by which the animal faſtens himſelf to bodies, however ſmooth, when it is tired of ſwimming, become evident, as likewiſe the veſtige of the aperture of the mouth, and the rudiments of the gills.

XVI. At firſt the tadpole does not ſhew any ſigns of animation, when touched with a needle, or ſuddenly expoſed to the rays of the ſun, even when concentrated in the focus of of a lens, he does not become ſenſible to theſe impreſſions, till his organs are further unfolded: he then gradually begins to move and looſen his fetters: at this time it appears clearly, that the internal circular membrane is the amnios, in the liquor of which the tadpole floats; the umbilical chord at laſt is ſeen, and becomes ſtill more perceptible, the firſt day after the animal has quitted his confinement. The cord is not, as in other animals, attached to the belly, but to the region of the head. A tadpole at the beginning of its evolution, is repreſented at N° II. fig. 2. and N° I. fig. 3. in which at 9 appear the little gills. At N° II. fig. 3. is a tadpole, magnified with its back turned towards the obſerver. In that at N° III. fig. 3. and thoſe fig. 4. is ſeen the veſtige of the eyes, and the umbilical cord coming out from below the mouth.

XVII. The reader will probably be ſurprized at this deſcription, whence it appears, that the tadpole does not come out of the egg, but that the egg is tranſmuted into a tadpole; or, to ſpeak more philoſophically, that the egg is nothing but the tadpole wrapped up and concentrated: in conſequence of fecundation it is evolved, and aſſumes the lineaments of an animal. Theſe phænomena were new and unexpected, for I was firmly perſuaded, that the globules of two colours, ſurrounded by mucus, were real eggs; all who have written concerning the generation of frogs, as Jacobeus, Valiſneri, and Roeſel, having ſo denominated them. But as greater deference was due to what nature ſhewed me ſo plainly, than to the authority of the moſt celebrated

celebrated writers, I am obliged to call theſe globules tadpoles or fetuſes inſtead of eggs; for it is improper to name any body an egg, which, however cloſely it may reſemble one, takes the ſhape of an animal without leaving any ſhell, as is the caſe with all animals that come from an egg.

XVIII. But it was of importance to examine by experiment theſe globules before fecundation, and while yet in the uterus. Upon the moſt rigorous and exact compariſon it appeared, that not only the ſpheres of mucus exactly reſemble each other in ſize and nature, and the two membranes with reſpect to their poſition, ſhape, and colour, but that the unimpregnated globules are not by any means diſtinguiſhable from the impregnated ones. Upon ſeparating the mucus and the two membranes, they appear to be alike ſpotted with white and black, which colours remain in ſome meaſure when the evolution of the tadpole is complete. The perfect ſimilitude, as well external as internal, is very ſtriking. If a globule be pierced with a needle, either before or after fecundation, a ſemifluid yellowiſh white ſubſtance oozes out: if the aperture be enlarged, the whole internal capacity appears to be filled with this matter, which loſes its fluidity as the tadpole is evolved, every day acquiring greater conſiſtence. If the globules before or a little after they come out of the uterus be put into ſpirit of wine or vinegar, or be boiled, and then ſtripped of their pellicles, we ſhall find ſo many indurated maſſes, which when cut through appear to be homogeneous. If we next proceed to examine the external parts, the pellicle before fecundation is thin and tranſparent, which properties it retains. Afterwards, indeed, as the tadpole is evolved, it enlarges and thickens juſt as the ſkin of other animals grows with their growth. As the pellicle is made to adhere to the inſide of the globule by heat or vinegar, ſo it continues to adhere after fecundation; and it adheres more and more firmly as the globules loſe their ſhape, and acquire that of the tadpole.

XIX. Hence the identity between the impregnated and unimpregnated globules is manifeſt. But the former are nothing but fetuſes of the frog, therefore the latter muſt be ſo too, and conſequently the fetus exiſts in this ſpecies before the male performs the office of fecundation. By this we are led to other conſequences of no leſs importance. 1. As theſe ſuppoſed eggs exiſted in the ovarium before their deſcent through the oviducts into the uterus, and long before fecundation (IV, VIII), the fetus exiſted in the mother's body long before fecundation. 2. Although the evolution of theſe fetuſes is never ſo conſiderable and quick, as after fecundation, it is, however, remarkable before; for let it be conſidered, that the fetuſes in the uterus are above ſixty times larger than they were a year before, when they adhered to the ovarium, as I have found by comparing them. 3. Beſides the fetus, the amnios and umbilical cord exiſt before fecundation.

CHAP.

CHAP. II.

GENERATION OF THE TREE-FROG.

XX. IN this and the following chapters, I ſhall be brief, and ſhall only, to render the hiſtory of theſe animals, deſcribed in them, more compleat, make a few additions to Roeſel's account, and notice ſome miſtakes into which he has fallen.

The tree-frog is of very ſmall ſize. The back is of a very beautiful green colour. A viſcid juice, that oozes out from its feet, enables it to faſten itſelf to the ſmootheſt bodies. During the fine ſeaſon, it fixes its abode upon trees, except in the ſpring, when it deſcends into ſtagnant water, in order to propagate the ſpecies.

XXI. Though the male mounts upon the back of the female, and embraces her very cloſely, he does not, like the green aquatic frog, throw his arms round her thorax, but faſtens his hands under her axillæ. Hence the females eſcape thoſe accidents that happen to the females of other ſpecies, which are often killed before parturition, by the violent ſtraining of the male, as appears from the contuſions and lacerations upon the breaſt.

XXII. The copulation of this ſpecies continues ſometimes, according to Roeſel, three whole days, though at others it is finiſhed in one day. Thoſe which I obſerved did not continue together more than a few hours. This difference I attribute to the warmer temperature of our climate, which occaſions more ſpeedy parturition.

XXIII. The ſame naturaliſt obſerves, that during copulation, the female plunges at intervals into the water, and remains immerſed ſome time: during which, the male approximates the extremity of his body to that of the body of the female, and that this motion is always performed with greater briſkneſs, while the eggs are coming forth. He confeſſes that he could never, with all his attention, perceive the part that characteriſes the male, nor any liquor emitted for the fecundation of the eggs.

XXIV. Though I could never diſcover any protuberance or papilla, which I could ſuppoſe to be the penis, yet by taking the animals out of water (XII), I have been able to throw further light on this intereſting topic. I obſerved, that about half an hour before any eggs are diſcharged, the ardour of the male became extreme. He would frequently ſtretch out the poſterior part of his body, and then bring it into contact with that of the female. He would then remove it, and ſoon apply it again. Theſe motions became more frequent when the eggs began to be diſcharged: and continued as long as they continued to be brought forth.

XXV. During this proceſs, I could not perceive any emiſſion of ſemen; but upon lifting up the hind part of the male, I ſaw a tranſparent liquor ejected from the anus at intervals, which I readily concluded to be the ſemen. When I made uſe of the breeches (XIII), the eggs never produced tadpoles:

and

and the breeches were wetted with ſemen as before (XIII).

XXVI. Are the eggs in this ſpecies alſo impregnated only after they are diſcharged? I obſerved a ſlight variation in the preſent caſe. The eggs of the uterus neareſt the anus, are ſometimes impregnated before they are diſcharged. But the ſingularity of the phænomenon requires that I ſhould be a little more explicit. Roeſel truly obſerves, that in the female of this ſpecies, the eggs deſcend into the uterus before ſhe is embraced by the male: if they are now taken out, and ſet in water, they all putrefy. This alſo happens if they are taken out after they are coupled, but before the female has begun to diſcharge any eggs. But when the diſcharge has commenced, if the female be opened, and the eggs of the uterus, and thoſe near the vent, is put into ſeparate veſſels of water, ſeveral of the latter will ſometimes be found to be productive, whereas all the former will ſpoil. This is a clear proof, that the ſemen ſometimes penetrates a little way into the body, whether it is projected ſo far by the male, or rather whether the eggs already fecundated, after their diſcharge, are ſometimes retracted, when the female is unexpectedly ſeized by the obſerver, and ceaſes to expel any more.

XXVII. The eggs are ſmall in proportion to the ſize of this ſpecies: one hemiſphere is yellow and the other black: when they are taken out of the oviducts or uterus, they are enveloped with gluten, which circumſtance has unaccountably eſcaped the obſervation of Roeſel, who aſſerts, that the gluten is not viſible till the eggs have been twelve hours in water.

XXVIII. Every egg is ſurrounded by a ſphere of mucilage, as in No. I, II, III. of fig. 5. pl. 1. The ſphere and egg undergo the following changes in water. They increaſe in bulk, and at the ſame time the latter is elongated, and preſſes againſt a circular membrane, which lies within the mucus and ſoon pierces it; as the elongation increaſes, the hole is enlarged, till at laſt the membrane is divided into two concave ſegments: within which appears a ſecond circular membrane, of much finer conſiſtence, and of courſe leſs apt to ſtrike the eye. This membrane, when it is broken, emits a quantity of liquor, and is no other than the amnios of the tadpole, as afterwards more plainly appears. Meanwhile the two ſegments ſeparate entirely, or at leaſt cohere but at a few points, and part from the amnios without growing any more; but the amnios becomes thicker, and many times larger than it was at firſt; as alſo does the egg, on the growth of which depends that of the membrane. The gradual enlargement is as follows:

XXIX. When it has become ſomewhat longer and bigger, one extremity thickens, and the other becomes thinner. While the obſerver is intent on theſe changes, he is ſurprized with the motion of the egg; it ſometimes turns round, now and then writhes itſelf, ſometimes it brings together its two extremities, and then ſtretches itſelf and returns to its original poſition. To theſe unexpected appearances another equally unexpected

pected succeeds: from the larger end two processes sprout and suggest the idea of the protuberances, by means of which the tadpole attaches itself to bodies (XV); above these appear two tumours, which recall to the memory the shape of eyes; they appear distinctly in a day or two, and it then becomes necessary to substitute the notion of a tadpole in the place of that of an egg. Let the reader cast his eye on No. IV. Fig. 5. and he will see a sphere of mucus, the inclosed membrane and the tadpole not yet evolved. No. I, II, III. Fig. 6. represents the membrane dividing into two segments, which when they are entirely parted, leave the tadpole No. IV. at liberty, which appears bent No. VI. Meanwhile the amnios is seen in the dotted circle I. full of liquor, in which the tadpole floats. The two segments are still plainer at Fig. 7. where two amnioses are seen distinctly, one of which is entirely separated from the segments.

XXX. But do these tadpoles exist before fecundation as in the foregoing species? To determine this question, I had only to make a rigorous comparison between the supposed eggs, after and before impregnation, and I analysed them in the same exact and careful manner as I had others (XVIII), and found the most perfect resemblance between them; whence it was necessary to conclude, that the fetus exists in the female of this species also before impregnation.

XXXI. These fetuses are further evolved, though still inclosed in the amnios, where they continue longer than those of the green aquatic frog; the time is commonly six or ſeven days; then the rudiments of the gills begin to ſhoot. At firſt they ſwim ſlowly, but as their limbs acquire greater ſtrength and vigour, their motions become briſker. Fig. 8. repreſents two tadpoles at the time of quitting the amnios, magnified by the microſcope: their gills are ſeen at A A.

XXXII. Upon comparing theſe obſervations with thoſe of Roeſel, I cannot but diſſent from him. The ſum of what he ſays on the generation of the tadpoles of the tree-frog is as follows.

XXXIII. At firſt the impregnated eggs grow in bulk only; then the tadpoles, which now ſeem to conſiſt chiefly of belly, though the head and tail may be clearly diſcerned, appear diſtinctly. Every tadpole floats in the white (the mucilage that ſurrounds it XXVIII). The belly is formed by the yolk, ſo the author calls that which is generally denominated the egg. At the tail appears a veſicle, and afterwards another (from his figures theſe ſeem to be the two ſegments XXVIII); theſe veſicles then ſeparate, and one remains at the head and the other at the tail. Roeſel conjectures, that the tadpole now endowed with motion and life, takes its nouriſhment from the veſicle at its head, but he confeſſes that he does not know the uſe of the other.

XXXIV. Though it is true, that the tadpole has at firſt a large belly, it is falſe, or at leaſt a very improper expreſſion to ſay, that the belly is chiefly formed of the yolk, ſince the yolk is the animal itſelf (XXIX); it would not be leſs abſurd to ſay, that the

belly

belly of an animal confifts of the animal itfelf.

XXXV. The defcription of the appearance of the two veficles, fhews that he did not make his obfervation with fufficient diligence: he probably made it at intervals; otherwife he muft have perceived that they were originally a fphere, which is divided into two fegments by the efforts of the tadpole; confequently they did not appear one after the other, but both exifted at the fame time (XXVIII).

XXXVI. If he had taken them up with a forceps, he would have found, that they cannot be fuppofed to nourifh the tadpole, for as foon as they appear, which is when the fphere is divided, they may be removed without injury to the young animal.

XXXVII. The part really neceffary to this animal, and of which the removal is fatal, is the amnios, within the liquor of which it at firft floats (XXVIII, XXXI). As Roefel does not mention the amnios, it muft have been unknown to him; and although this membrane is found in every fpecies of frog and toad, not the leaft mention is made of it in his hiftory; this circumftance the more furprized me, fince it did not efcape the diligence of Swammerdam, in whofe footfteps the German naturalift profeffes to tread.

XXXVIII. He has, however, remarked an accident, which happens to tadpoles that die in the amnios, or as he expreffes it to eggs, that are not prolific: they become fometimes pointed, fometimes pear-fhaped, fometimes oblong, and at others narrow in the middle, and ſo on. Four ſuch tadpoles are repreſented in Fig. 5. at ABCD.

XXXIX. The tadpoles of this ſpecies require great attention. They muſt be kept, not merely in clean water, but in that which is taken from the places where the males and females meet, as from ditches and puddles (XX). This precaution was not abſolutely neceſſary for other frogs and toads; well water being ſufficient in theſe caſes.

CHAP. III.

GENERATION OF THE TOAD, DENOMINATED BY ROESEL, BUFO TERRESTRIS DORSO TUBERCULIS EXASPERATO, OCULIS RUBRIS.

XL. THOUGH Roeſel mentions but one ſpecies of this ugly and diſguſting animal, I incline to believe, that in this country there are two; this opinion is founded on a difference in the external configuration; not to mention a difference in colour, ſome having their backs and ſides of a light cineritious hue, with tubercles of a tan colour, and others of a pale green, with tubercles of a dark red. But notwithſtanding this difference, I ſhall ſpeak of them as one ſpecies, for in what relates to generation, there is the moſt perfect ſimilitude.

XLI. Of the whole claſs of frogs and toads with which I am acquainted, none begin their amours ſo ſoon as the ſpecies in queſtion: they are found coupled in the beginning of March, and ſometimes in February, before all the ſnow is melted, and while the water in many places is yet covered with ice. The male being about five times ſmaller than the female, cannot nearly bring the toes of his fore feet to meet over the breaſt, a circumſtance before noticed in the green water-frog (VI). He, however, holds her ſo faſt, that ſhe can by no means diſengage herſelf from his embraces. The females have often got out of the veſſels in which I kept them, and eſcaped to a conſiderable diſtance, but the males ſtill kept their ſituation.

XLII. The eggs, as well as in toads as frogs, lie at firſt in the great ſacs of the ovaria, whence they get firſt into the oviducts, and then into the uterus. In which of theſe three ſituations are they in the female toad with red eyes, and dorſal tubercles, at the beginning of her amours? I have found that they are indeed in the ovaria, but ready to paſs into the oviducts. Copulation laſts till they are diſcharged, which happens in ten, twelve, or fourteen, and ſometimes twenty days, if the ſeaſon be cold. Such is the ardour of the males, that after the diſcharge is finiſhed, and they have quitted the female, they will return to her again, and embrace her for ſeveral hours. This, however, is not peculiar to toads. I have often ſeen it happen in frogs.

XLIII. The male, during the whole time, makes a kind of grunting noiſe, which I

never

never heard except at the ſeaſon of his amours. This noiſe becomes louder, whenever attempts are made to pull him away, or when any other toad comes near; which he moreover obſerves with a jealous eye, and endeavours to drive away by throwing out his hind legs.

XLIV. If the belly of the female be attentively obſerved after ſeveral days of copulation, it will be ſeen to move in a very extraordinary manner: it ſometimes dilates towards the breaſt, which dilatation paſſes gradually downwards; at others the dilatation begins at the bottom of the belly, and proceeds upwards. During theſe agitations, the female, now about to bring forth, evidently ſuffers conſiderable pain, probably on account of excluſion of the eggs from the ovaria, and their paſſage through the windings of the oviducts, and into the large cavity of the uterus; for the two laſt mentioned places will be found to contain eggs, if the female be opened during theſe alternate ſwellings.

XLV. In frogs the eggs are ſoon diſcharged; but in toads this is a very tedious proceſs. Two cords, conſiſting of a viſcid tranſparent matter, and containing a number of black globules, which are the eggs come out from the vent, and are excluded ſo ſlowly, that the eye cannot diſtinguiſh any movement in them; but in a few hours they meaſure ſeveral feet.

Theſe cords, conſiſting of eggs and gluten, certainly receive their form in the oviducts, as in a preſs; which may be ſeen upon opening the female, as ſoon as they have advanced a little way into thoſe canals. The time requiſite for the

diſcharge

discharge is quite uncertain; the longest I have observed was finished in thirty, and the shortest in nine hours; it lasts generally above twelve. A piece of these cords is represented at Fig. 9. Plate II. Fig. 10. shews a smaller portion magnified.

XLVI. Two entire cords measured forty-three Paris feet; the number of eggs amounted to one thousand two hundred and seven: hence we cannot be surprized that the female, after the exclusion of the eggs, should become so much smaller.

XLVII. Since the ardour of the males is so great, that they do not abandon the females, even when they make their escape (XLI), it was natural to suppose, that they would also continue their embraces when I removed them from the stagnant or slowly running waters, where they assemble towards the end of spring, in order to propagate the species (XLI). to a dry and confined place, where I could watch the manner in which fecundation takes place. I will relate what I observed during many long and tedious inspections.

XLVIII. As soon as the female begins to discharge the mucous cords, the male, that before lay close and collected, spreads himself, and stretches his hind legs backwards towards those of the female, still holding her breast close with his fore legs. This change of posture enables him to reach the cords with the extremity of his body, and to bedew them with semen.

XLIX. And this is not done by a forcible emission, but by simple contact; for whereever the cord is touched, it appears just moistened,

moiſtened, as if with a pencil dipped in ſome fluid.

L. The male, after having repeated this many times, with various contortions, becomes weary, collects his body, and draws his hind legs cloſe to his belly. Meanwhile a freſh portion of cord is diſcharged, and then the male reſumes his employment, and afterwards again returns to his repoſe. And thus he goes on, till all the eggs are impregnated.

LI. That the act of fecundation conſiſts in this approximation of the extremity of the body to the eggs, and bedewing them, appears from the following proofs: Firſt, if the female be killed while ſhe is diſcharging the eggs, of thoſe which lie in the uterus and oviducts, not one will be prolific, though they be kept in proper water; whereas almoſt all the others that have been moiſtened with the ſeminal fluid, at their excluſion from the vent, will grow. Secondly, If during the diſcharge the irroration ſhould be interrupted, either by means of breeches, (XIII). or by the near approach of the obſerver, (for then the male through fear will bring his hind legs and thighs cloſe to his body) the portion of eggs then excluded will be barren, while the reſt will be found to have been fecundated.

LII. Theſe laſt facts clearly evince, that in theſe animals likewiſe fecundation takes place without the body of the female.

LIII. But may not the bodies, which have hitherto been called eggs, be tadpoles not yet unfolded? and if ſo, do they exiſt before impregnation, as in frogs (Chap. I. and II.)?

Theſe

Theſe problems were yet to be ſolved, and required a ſhort analyſis of the eggs.

LIV. They are black, and, to the naked eye, and even when a weak magnifier is uſed, appear to be globular (Fig. 9, 10. Plate II). when examined with a glaſs of greater power, they ſeem to be marked with four furrows, which interſect each other at right angles, nearly like the huſk of a cheſtnut half opened, (Fig. 11.): though the furrows are not bare, but covered with a very fine tranſparent membrane, which paſſes very tight round the reſt of the egg. When this is taken away, we come to the ſkin of the egg; it is black, and is lacerated by the ſlighteſt touch; as is alſo the inſide, which is almoſt fluid, of a browniſh white colour, and apparently homogeneous; the particles appear globular, when obſerved by the microſcope.

LV. Such is the form and compoſition, as far as the eye can diſcover it, both of the eggs that have been diſcharged and fecundated, and of thoſe that are taken from the uterus and ovaria; excepting, that the latter are deſtitute of the mucous gluten, ſmaller, and not ſo black.

LVI. Wherefore, if we rely upon the information of the ſenſes, we muſt conclude, that there is no eſſential difference between the impregnated and unimpregnated eggs.

LVII. If we obſerve the changes produced in the former, as we have before done in frogs, we ſhall find that they are immature fetuſes, and exiſt before impregnation. They ſoon aſſume the form of thoſe little animals; firſt becoming thicker, then longer, and afterwards growing thin at one extremity, viz.

the

the tail, and becoming round at the other; from which ſpring two appendixes, viz. the gills; and in theſe the circulation of the blood is very conſpicuous, as well as at the edge of the body. At Fig. 14. may be ſeen ſeveral of thoſe little bodies, loſing the ſhape of an egg, and aſſuming that of the tadpole; they are further advanced at Fig. 15. Fig. 12. ſhews ſome of theſe corpuſcles, loſing indeed the form of the egg, but not aſſuming that of the tadpole, and ſpoiling; this appears more at large at Fig. 13: theſe were never ſubjected to the influence of the fecundating fluid.

LVIII. While the tadpole is thus evolved, the thin membrane, of which we ſpoke at LIV. is enlarged in proportion; this membrane is the amnios, and contains a liquor, in which the fetus is ſeen to float; it may be found in the uterus (LIV, LV). and therefore exiſts before impregnation.

LIX. Though the blood circulates in the fetus (LVII). and the heart muſt therefore beat, yet not the ſmalleſt motion can be excited, either by puncture, or any other mode of irritation; it is not till the organs have acquired ſome conſiſtence, that the animal begins to move; in a few days after this period it burſts the amnios, and ſwims about.

CHAP.

CHAP. IV.

GENERATION OF THE FETID TERRESTRIAL TOAD.

LX. THIS ſpecies, which does not much exceed in ſize the green aquatic frog, (III). agrees in ſome particulars with that denominated by Roeſel Bufo terriſtris fœtidus, but differs in ſo many others, that it muſt be reckoned a ſpecies totally diſtinct. They are both nearly of the ſame ſize, and when irritated emit an offenſive ſmell; they moreover agree in colour, (the belly being of a white, inclining to a green in both, and the back to yellow); if we except a liſt of a golden yellow colour, which, in the ſpecies of Roeſel, runs along the ſpine, but in mine does not appear. But the eſſential difference conſiſts in the abſence of the web that connects the toes of ſo many other amphibious and aquatic animals beſides frogs, from the ſpecies deſcribed by the German naturaliſt; in that of which I am about to treat, it is very viſible, and of very great ſervice in ſwimming. The ſecond difference lies in the progreſſive motion upon dry ground, thoſe of Roeſel advancing without riſing from the ground; *muris inſtar* is his phraſe; whereas mine leap like frogs. Laſtly, a difference of ſpecies may be deduced from the difference of the voice, which, in the former, is exactly like that of the green tree-frog, and conſiſts

consists of a loud screaming, but in the latter is a kind of continued whistle.

LXI. The female, in the two other species of terrestrial toad, is five times as big as the male (XLI). But in this species the male is scarce one third smaller than the female, when she is about to exclude the eggs; afterwards they are nearly of the same size. They also agree in colour; but the upper part of the body of the female is adorned with cineritious spots and lists, from which arise some very beautiful red tubercules.

LXII. Toads of this species live on dry ground all the year, except during the season of copulation; at which time they repair to the water of pools, ponds, puddles, ditches; never to rivers, torrents, or quick currents.

LXIII. They repair to the water when the spring is pretty far advanced, in May, or at the beginning of June. Their amours last above a month; not that the male embraces the female so much longer than others, the contrary is rather true; but, during all this time, pairs may be found coupled; some beginning earlier, and others later, according as they are instigated by their internal feelings. The same thing happens in the two other species of toad, the aquatic and tree-frogs, fishes, and all animals that have a fixed season for their amours.

LXIV. By comparing paragraph LXIII. with XLI. the reader will perceive the great difference of time at which the two other species of toad, and that which is the subject of this chapter, begin to propagate; whence we may infer, that they are naturally different.

The

The former begin early in March, as ſoon as the rigour of winter is a little abated; whereas the latter requires a warm temperature. If the weather ſuddenly changes, the fœtid terreſtrial toad abandons the buſineſs of generation, and retreats to dry ground. I was eyewitneſs of a ſingular fact which may be related on this occaſion. At the end of May, 1777, I perceived upon the ſurface of a large pool great numbers of this ſpecies, ſome of which had diſcharged their eggs; others were diſcharging them, and others were only juſt coupled; all were in motion, and ſo intent upon their employment, that they ſuffered themſelves to be taken, without attempting to eſcape. The males, like the green aquatic frogs, claſped the breaſt of the females very tight with their fore feet, and made a low grunting noiſe, not unlike that of the two other ſpecies (XLIII). The ſky was clear, and in the ſhade the mercury ſtood at 16° (*a*), when an unexpected rain fell for two days, and cooling the air, diſperſed all the toads, ſo that when I came on the ſecond day of the rain, to catch ſome for my experiments, to my ſurprize I could not find one. I ſoon, however, was aware that they had quitted the water, to conceal themſelves in the crevices of a neighbouring wall, where they continued till the atmoſphere becoming warmer, invited them again to the pool.

LXV. It is the opinion of many naturaliſts, that the male toad embraces the female for forty days; and ſome think that this is the

(*a*) Sixty-eight of Fahrenheit.

caſe alſo with the frog (*a*). Having obſerved that, in like circumſtances, the duration of copulation is inverſely as the heat of the atmoſphere, I can eaſily believe that this may be the caſe in cold countries. In temperate climates, as with us, it is far otherwiſe. The tree-frog embraces the female a few hours (XXII), the aquatic about a week (VI); the two former ſpecies of toad longer, on account of the early commencement of their amours. It will therefore eaſily be believed that, for a contrary reaſon, the embraces of the ſpecies in queſtion do not laſt long, four or five days for inſtance.

Nothing can be more ſalacious than the male. He not only adheres moſt tenaciouſly to the female, but does not even quit her for ſome time after the excluſion of the eggs. If he is ſeparated by force, he returns to his ſituation immediately, though kept in a narrow veſſel, and under the eye of the obſerver.

LXVI. About the ſeaſon at which theſe animals meet, I procured a large quantity, and ſet them in veſſels almoſt full of water, a male and female in each veſſel. In ſome I put only a female. They were ſoon coupled, ſome ſooner and others later. In two females that were opened twelve hours after the male had begun to embrace them, the eggs had not yet quitted the ovaria; in one that had been coupled two days and a half, the ſame phænomenon occurred. In another, that was opened at the end of the third day, ſome eggs had entered the oviducts, and thoſe neareſt the uterus were ſurrounded by mucus. Before the concluſion of the fourth

(*a*) Swammerdam Bibl. Naturæ.

day, the eggs of two other females had all descended into the uterus, and were completely ſurrounded by gluten. The ovaria now contained only ſmall and immature eggs. Theſe obſervations were made upon females embraced by the male; though the ſolitary ones were kept for a fortnight, the eggs continued in the ovaria.

LXVII. While I was engaged in theſe experiments, a ſingular phænomenon occurred, which muſt not be paſſed over in ſilence. Of two females that were opened on the fourth day of copulation, I found the eggs of one in the abdomen, and of the other partly in the abdomen, and partly in the thorax. This accident may eaſily be explained. The eggs were without gluten, and therefore could not have paſſed through the long windings of the oviducts, and had either never been taken into them, or the ovaria perhaps burſt, and hence they would fall both into the ſuperior and inferior cavity. Such a rupture may eaſily be conceived to happen, if we conſider the preſſure made by the male againſt the region of the breaſt oppoſite the ovaria.

LXVIII. I have already ſaid, that the eggs about to be excluded having paſſed through the canal, provided by Nature for that purpoſe, were found in the uterus on the fourth day (LXVI). In the greater part of thoſe that were confined in my veſſels, they began to make their appearance at the vent on the 5th day in the form of cords, as in the other ſpecies (XLV). Theſe cords continue to be very ſlowly expelled till the uterus is empty, which always happens before the ſeventh day. Meanwhile the male brought the ex-

 tremity

tremity of his body towards them, from which iſſued a ſmall jet of liquor; this was the ſemen, as evidently appeared from thoſe portions of cord that were ſprinkled with it, bringing forth tadpoles, whereas the others ſpoil and turn putrid. In like manner the whole length of the cords produced nothing when the male was cloathed with breeches (XIII), or parted from the female as ſoon as the eggs began to be excluded.

LXIX. As the eggs that were not moiſtened with the ſeminal fluid after they were diſcharged, were unproductive (LXVIII), I may confidently infer, that fecundation takes place without the body of the female. I have moreover frequently taken pieces of cord out of the uterus, and placed them in water, but never did I ſee one egg productive; this obſervation is likewiſe applicable to the eggs that were deſcending through the long windings of the oviducts. Hence might be inferred the phyſical impoſſibility of the eggs which were ſhed in the cavities of the thorax and abdomen being prolific (LXVII), as experiment deciſively proved.

LXX. The cords, upon examination, appear to conſiſt of an almoſt innumerable ſeries of eggs, each at a ſmall diſtance from the other: they are connected by gluten, which always encloſes the fetuſes of the frog and toad, and which is prepared in the cavity of the oviducts, for in them the fetuſes, before naked, are inveſted with it. The great reſemblance between the phænomena exhibited by the tadpoles that I am about to deſcribe, and by thoſe already deſcribed [illegible] (Chap.

(Chap. III), renders a reference to figures unneceſſary.

I was curious to know, whether the gluten is neceſſary to the growth of the egg, I therefore removed it entirely from ſome, and from others only in part, without injuring the amnios, which in this as well as in other ſpecies of frogs and toads, is very conſpicuous. Thoſe which were quite ſtripped of their gluten, never brought forth young, but the others, to which a portion was ſtill left adhering, were prolific. When the amnios began to ſpoil, it was a certain ſign, that the egg, though yet ſound, would ſpoil.

LXXI. In this ſpecies the little bodies, which we have hitherto called eggs, are tadpoles in miniature, as the following proofs may ſerve to ſhew. They are not broken like other eggs, to allow the incloſed animal to paſs out, while they themſelves become an empty and uſeleſs ſhell, but undergo no other change than an increaſe of bulk, becoming ſlender at one extremity, at which the tadpole ſoon appears, and round at the other, which the eyes, the mouth and gills ſoon ſhew to be the head; at length life appears in theſe fetuſes, they break through the amnios and ſwim about the water; juſt as the fetuſes of the other frogs and toads, which likewiſe have at firſt the ſhape of eggs. The advanced ſeaſon cauſes the tadpoles of this ſpecies to exhibit their different *phaſes* in leſs than three days, whereas others being brought forth at an earlier period, require a longer ſpace of time.

LXXII. During the evolution, I analyſed theſe corpuſcles with the utmoſt care, and compared them both internally and externally with others in the uterus and oviducts, but could perceive no difference except in ſize. From this identity then it may be concluded, that as theſe corpuſcles are real tadpoles without the body of the female, they are ſo alſo within it, and by conſequence, that the fetus exiſts in the female before the concurrence of the male.

LXXIII. This is alſo true of the amnios, for I have found that membrane ſurrounding the fetus, when in the uterus; it is then, indeed, in contact with the fetus, and does not contain any liquid; when they are excluded, it is at ſome diſtance, and full of its proper fluid. In like manner there is reaſon to believe, that the umbilical cord exiſts before fecundation. This organ is indeed not viſible, when the tadpole floats in the liquor of the amnios, yet it probably exiſts, ſince it is attached by one end to the lower part of the head of the animal, and by the other to the amnios, from which it afterwards ſeparates, and adheres a little while longer to the tadpole; it then decays by little and little, and is at laſt quite obliterated.

LXXIV. I have obſerved, that in order to rear theſe animals, I immediately put them into water, imitating in this the example of the female. I added, that except for the tree-frog, well water would ſuffice, that ſpecies requiring it to be taken from certain ponds or ditches (XXXIX). To theſe precautions, I muſt ſubjoin two others, for the ſake of thoſe who may be deſirous of repeating my experiments. Firſt, The veſſels muſt not be kept in too warm a place, cold being less hurtful in these experiments than heat: the two first species are produced before the close of winter, and the water of the vessels in which I kept them has been frozen to a considerable depth, without doing any hurt to the tadpoles. I twice increased the cold, so that almost all the water was frozen, but the tadpoles never suffered, except when the mucus itself, in which they were enveloped, was frozen; which never happens, but in a degree of cold far exceeding the point of congelation. On the contrary, whenever I left them some hours exposed to the sun, when the thermometer stood at 35°, the greater part was sure to perish.

The next precaution, not less important than the preceding, consists in often changing the water of the vessels; if this be neglected, evolution will not begin, or if begun will stop; this happens in consequence of the gluten becoming putrid, from which a penetrating miasma insinuates itself into the tender and delicate fetus, and destroys it either before it shews any signs of life, or after it has given tokens of animation.

In order to observe exactly the amours of confined frogs and toads, it is necessary to place something to obstruct the view of these salacious animals, between the vessel and the observer, otherwise they are disturbed at the sight of a person standing over them, and either do not begin to celebrate their nuptials, or leave them half-finished.

Nice and fastidious persons may perhaps be disgusted at the frequent mention of an animal so loathsome as the toad. They would doubtless have been better pleased with remarks on ſuch creatures as are more familiar, and may be handled without abhorrence. But whoever purpoſes to explore Nature, conceives not diſguſt or predilection for any of her productions, but examines all indifferently, well knowing, that the ideas of what is *diſagreeable*, *nauſeous*, &c. are not in Nature but the imagination; for every creature, how vile and horrible ſoever it may appear, has its proper beauty and grandeur, and is the proper object of admiration, ſince it is the work of the ſupreme Architect. This did not eſcape the ſage Petrarch, who ſings

> Tutte le coſe di che 'l Mondo è adorno
> Uſcir buone di man del Maſtro Eterno.

LXXV. I will cloſe the chapter, by noticing a fact mentioned by the illuſtrious Bonnet in his *Corps organisès*, and connected with the preſent ſubject. He tells us, that he knew a pool, which, after remaining dry, for ſome time, was ſtored with the ſame kind of fiſh that it contained before, nor could he diſcover how they came into it. Inſtead of having recourſe to fiſhes accidentally dropped by cranes for the explanation of the phænomenon, he conjectures, that the ſupply came from fecundated eggs that had lain in the mud without being ſpoiled. The inſtance of a ſpecies of tufted polype, diſcovered by the immortal Trembley, of which the eggs may be hatched by putting them into water, after they have been kept four months in the dry, affords a remarkable foundation for his conjecture; which he further confirms by the famous worms of ricketty corn; theſe, it is well known, after

having

having been dried for many years, will move again as well as at firſt, if they are barely wetted. Deſirous, however, on this as well as on all other occaſions, of aſcertaining the truth, the philoſopher of Geneva wiſhed that his conjecture might be ſubjected to the teſt of experiment, and that it might be tried, whether eggs of fiſhes can be hatched after having been kept ſome time in the dry.

As ſoon as I had read the work, I made the experiment, but without ſucceſs. Of the eggs of three ſpecies of fiſhes, which I kept in the dry for a month, not one brought forth young; indeed this appeared to be impoſſible, for moſt of the eggs were deformed and ſpoiled by deſiccation. Several years afterwards, being engaged in experiments relative to the generation of toads and frogs, I put tadpoles recently excluded to the ſame trial, and with the ſame ill ſucceſs. Hence it appears, that in order to explain the phænomenon related by Mr. Bonnet, we cannot have recourſe to the ſuppoſition, that eggs may be long kept in the dry. And I am ſtill more inclined to this opinion, from having obſerved, that the eggs of fiſhes and tadpoles, newly laid in a ſhallow place, periſh irretrievably if they continue in the dry for a few days, notwithſtanding they are afterwards put in water. I ſay for *a few days*, for tadpoles do not die if they continue but a little while out of water. I put ſome tadpoles juſt excluded into a dry glaſs; for two or three days the evolution went on, they began to aſſume their true ſhape, and even to ſhew ſigns of life, but theſe appearances diminiſhed gradually, and at laſt they ſhrivelled up and became

came disfigured amid the mucus, which was itſelf turned into a kind of hard glue.

LXXVI. But how then are we to explain the appearance of fiſhes where there were none once, and where we know that freſh ones have not been brought? The purport of this diſſertation does not, as is evident, lead me to the ſolution of this phænomenon, which was only mentioned incidentally. And if I was obliged to treat of it, it would rather be to ſuggeſt doubts concerning its reality, than account for it. We know not how any fiſhes have got into places that were once dry, and are now covered with water, but is it therefore certain that they did not get there? Are we ſure that nobody put them there? Is it clear that they themſelves have not found ſome ſubterraneous way, or unperceived rill? Muſt we not of neceſſity draw ſome ſuch concluſion, ſince it is certain, on one hand, that the dried eggs of fiſhes are ſteril, and on the other, that they come only from eggs.

CHAP. V.

GENERATION OF THE WATER-NEWT, OR SALAMANDER.

LXXVII. MY obſervations were made upon two ſpecies of newt. One has the lower part of the body tinged with a very beautiful orange colour, interſperſed with

with black ſpots of an irregular ſhape; the ſides and back have a ferruginous ground with grey ſtreaks; the length of this ſpecies is four inches and upwards, and the thickneſs about ſix lines. The other ſpecies, beſides being ſhorter and leſs bulky in proportion, is diſtinguiſhed from the former by a liſt of a golden yellow, which bounds the ridge of the tail, and in females paſſes upwards along the ſpine, and reaches as far as the head. In both theſe ſpecies the males differ from the females, not indeed in the external parts of generation, which are alike in each ſex, but in being provided with a dentated, membranous prominence, running longitudinally along the middle of the back; as alſo in a double ſilver-coloured band, which adorns the tail of the male, but is not perceptible in that of the female. Theſe two ſorts of newts are the ſame which, in reſearches of another nature, were the ſubjects of thoſe beautiful experiments on the reproduction of the limbs, the circulation of the blood, and other intereſting topics, diſcuſſed in my other works already publiſhed.

LXXVIII. During ſpring and ſummer theſe little quadrupeds live in ſtagnant, never in running waters; at the approach of winter they conceal themſelves under ground, within the clefts of ſtones, and even in vaults; many of them, however, abide in the water during the whole winter, and more eſpecially in ſuch as ariſes from ſubterraneous ſprings, and preſerves a ſenſible degree of warmth in the keeneſt colds. Hence it appears, that our newts, though in general na-

tives

tives of water, and therefore denominated *aquatic* by me, are, however, capable of living, and in reality do live upon dry ground; and for this reaſon they may alſo be juſtly termed *amphibious*.

LXXIX. The comparative ſtate of the ſeaſon at which they diſappear from the waters (either deſcending to the bottom, or elſe tranſporting themſelves to dry ground, LXXVIII) in autumn, and of that at which they re-appear on the approach of ſpring, is worthy of notice. Before the middle of October, while in our climate the thermometer in the ſhade has ſcarce fallen ſo low as temperate, they all or almoſt all retire out of ſight, and are loſt; whereas, on the other hand, they begin to make their appearance, either by riſing to the ſurface of the water, or ſwimming in it towards the middle of February or thereabouts, although at this time it continues to freeze almoſt always during the night, and in the day-time the thermometer in the ſhade is ſeveral degrees below temperate. Now whence ariſes this early diſpoſition in newts to quit their retreats, while the winter is not yet over, whereas in autumn they go in queſt and take poſſeſſion of them, notwithſtanding the ſeaſon long continues mild? Does it ſpring from the ſtimulus of hunger produced by their long faſt during the winter months? Or may it more juſtly be imputed to another principle not leſs active than hunger, viz. that by which the individuals of different ſexes are determined to ſeek each other for the purpoſe of propagating the ſpecies? It is at leaſt certain, that in ſpite of the coldneſs of the ſeaſon, which, however,

however, prevents them from ſhewing ſuch vivacity, ſuch fire and alertneſs, as they afterwards diſplay, the males begin to chaſe the females, to ſurround and to careſs them in their manner, a circumſtance which is never obſerved except at the time of their amours.

LXXX. Theſe appearances in February are, however, only preludes to their future nuptials. Theſe are properly celebrated in March, and till about the middle of April; this however depends, as we have already ſeen in frogs and toads, on the greater or leſs warmth of the ſeaſon, which accelerates or retards their amorous encounters. Theſe encounters are conducted in this manner: the male purſues the female, which at firſt makes a ſhew of flying, but ſoon ſtops of her own accord. He then approaches her in ſuch a manner, that the lower part of his head comes in contact with the upper part of the head of the female; and this is done while the animals are in ſuch a poſture, that their bodies form an angle, of which the point is made by the union of the two heads. Sometimes, however, the poſition varies; ſome males, inſtead of placing their head on that of the female, only join muzzle to muzzle; the bodies of the male and female are always near together, ſo that the angle made by the two heads or muzzles, is always very acute. Then the male erects that dentated membranous prominence which lies on his back (LXXVII), agitates it in a ſtrange manner, and moves it from left to right, nearly as a mettleſome ſtallion waves his mane; the male alſo moves his tail briſkly, bends it in a tortuous manner, and as he waves it about, ſtrikes

ſtrikes very gently the ſides of the female, which continues without motion. Sometimes the male, in order to preſerve hi poſition, and at the ſame time to laſh the female with his tail, graſps with the toes of his fore-feet the graſs at the bottom of the ditch, or ſmall twigs, or whatever elſe will ſerve for a ſupport, remaining always immerſed in water along with his companion: and while he thus gently laſhes the female with his tail, he emits from the aperture of the anus, now unuſually tumid and dilated, a copious jet of ſemen, which mixes with the water, and thus diluted, arrives at the anus of the female, which likewiſe, on this occaſion, appears more enlarged as well as wider than uſual. In this important operation then, the anus of the male is never in contact with that of the female, which alſo always maintains a greater or ſmaller diſtance, and never ſhews any part at all characteriſtic of her ſex. After the male has ejected this jet of ſemen, he reſts for a while, on ſome occaſions quitting the female; he then returns to his employment, and repeats the contortions of the tail, and the emiſſion of ſemen. I have ſeen theſe alternations continue more than an hour; and when they are engaged in this employment, they may be approached and taken with the hand, almoſt without being aware of what happens to them. And ſometimes the male, from his agitations and contortions between the fingers, throws out a ſmall portion of ſemen, which is of a very white colour, and in conſiſtence reſembles thick milk.

LXXXI. I firſt obſerved theſe curious facts in places where the newts naturally aſſemble and ſport together, viz. in ponds, ditches, puddles, &c. afterwards I obſerved over again the ſame things at home in veſſels full of water, more commodiouſly and with equal ſucceſs, exactly in the ſame manner as I had obſerved the generation of frogs and toads. I found that this ſingular mode of impregnation takes place in both the ſpecies of newt which I have deſcribed (LXXVIII). Upon the whole it appears then, that copulation is not neceſſary to the fecundation of theſe animals, and I can aſſert with confidence, that among the many thouſands which I have obſerved at the time of their amours, I have never ſeen a ſingle pair in conjunction, not only for the purpoſe of generation, but on any other occaſion.

Being well aſſured, that with reſpect to exceptions from certain rules ſuppoſed to be general, diffidence and caution can never be carried too far, I was very deſirous of wiping away all ſuſpicion. I have been ſeveral times a ſpectator of the copulation of *wall-lizards*, which happens in April and May, and may be ſaid to laſt a ſingle inſtant. In the cleareſt days, and in places moſt expoſed to the ſun, the male runs after the female, and when he has overtaken her, he twines himſelf about her, and unites his genitals to her's, but this union may be termed momentaneous, after which the two ſerpents part. Do newts alſo, which on account of ſome reſemblance, are likewiſe called water-lizards, during their amours as deſcribed above (LXXX), do they alſo, I ſay, copulate for a ſingle inſtant? Although I kept my eyes fixed on them, ſo that the moſt trifling action or ſmalleſt motion

could

could not escape me, I am quite certain of having never seen them in copulation for a single moment throughout the whole period of their amours. I may even assert, that the genitals of the male were always at the distance of several lines from those of the female; and I have been still more and more convinced, that no copulation of any sort takes place in this kind of animals, but that fecundation is effected by that portion of semen which the male darts into the water, and which afterwards is received into the body of the female, at the aperture of the anus.

LXXXII. I made these observations in 1766 and 1767, and gave some intimation of them in my *Prospectus concerning animal reproductions*, published in 1768: my expressions are the following: "Naturalists are not certain whether newts copulate like the generality of animals, or in the manner of frogs and toads. As such an enquiry is of importance to the history of generation, I have paid great attention to it, and in my work will relate all that I have observed (*a*)."

I was persuaded that these observations were new, for in truth I was not apprized that any person had published any thing upon the subject. And I should still have remained under this persuasion, if I had not chanced to read in Bomare's *Dictionary of Natural History*, the article *Aquatic Newt*, in the volume printed in 1775, that is to say, seven years after the publication of my Pro-

(*a*) Page 77.

ſpectus, in which I found that Mr. Demours had turned his attention to the ſame point: I was not able to aſcertain whether his obſervations are prior or poſterior to my own, for want of knowing whether they are to be found in a ſeparate publication, or were ſimply communicated by the author to Bomare. However it may be, I acknowledge with the moſt unaffected ingenuouſneſs, that the painful feelings, which uſually ariſe on perceiving that another, without our knowledge, has begun to reap in our own field, have been amply compenſated by the pleaſure of finding that the obſervations of the French Naturaliſt perfectly agree with mine upon the whole, a ſtrong preſumption that the obſervers have been exact. The ſum of his obſervations is, that after various gallantries on the part of the male, ſuch as ſtopping the female, bending his body in the form of an arch in the water, and ſtanding upright with his creſt or prominence erected, he opens his anus, forcibly compreſſes the region of the teſticles, laſhes his companion with his tail, and now ſqueezing his teſticles with his utmoſt force, darts forth the ſeminal liquor, without however being in contact with the female. The liquor ſpreads along her ſides, after ſlightly clouding the water: Then the male drops down as it were aſleep, but ſoon awakes, and reaſſumes his former gallantries, which are ſucceeded by a freſh emiſſion of ſemen, after which the two animals ſeparate (*a*).

(*a*) Page 38, 39.

These obſervations, as far as they prove that no copulation takes place between newts, are confirmations of mine, whatever opinion is to be entertained concerning any ſlight difference in the accounts of the amorous geſticulations, as well as concerning the few emiſſions of ſemen obſerved by him, and the numerous ones remarked by me (LXXX): for with reſpect to the former, they are circumſtances totally foreign to the main object of attention, and as to the more or leſs frequent emiſſions of ſemen, theſe it is evident might depend on the difference of temperament, age, and vigour of the animals.

LXXXIII. In the LXXXIſt paragraph I have ſuppoſed that impregnation was effected by thoſe jets of ſemen which the male emits in the vicinity of the female. Let me now adduce the proof of this ſuppoſition; but that it may be better underſtood, it will be expedient to premiſe ſomething concerning the ovaria, oviducts, and ova of theſe animals. When the abdomen of a female ſalamander is laid open at any ſeaſon of the year, the two ovaria preſent themſelves, containing a multitude of little eggs of a yellowiſh white colour, ſmaller than huſked millet, and not floating looſe in the cavity of the ovaria, but adhering to their ſides. The little ova, at the approach of ſpring, increaſe inſenſibly in ſize, and when they are arrived at maturity, (which happens at the time of their amours) they deſcend into the oviducts, which conſiſt of two white tubes, which extend from the origin of the fore-feet to the root of the tail, and include the ſpine between them. Both are wrinkled and full of

curvatures

curvatures and flexions, when they are freed from theſe, and ſtretched in a right line, they exceed about four times the whole length of the animal.

LXXXIV. During the ſeaſon of amours, the oviducts always contain a greater or ſmaller quanity of eggs, placed in rows, and generally more numerous in that part of the duct which lies neareſt the anus. And now it is, that on ſlight preſſure on the belly, or even from the contortions of the animal, when they are held between the fingers, the eggs commonly come out from the cloaca, juſt as we have before obſerved, that the ſemen of the male is emitted under like circumſtances (LXXX). The ova, when they quit the ovaria and enter the oviducts, become far larger, and they now are inveſted with a viſcid gluten, nearly as happens in frogs and toads, except that the gluten which ſurrounds the eggs in the two latter kinds of animals is more abundant and more viſcid. When the male hath ejected the ſemen, and when from its vicinity to the female, it has inſinuated itſelf into her anus, that portion of eggs is fecundated, which is neareſt the vent, and about to be excluded, and the remainder which lies in the higher part of the oviduct, continues at that time unimpregnated. This truth actual experiment proved to me in the following manner. When the males began to purſue the females with eagerneſs, I put the latter by themſelves in veſſels full of water. They, notwithſtanding, diſcharged their ova, but they remained barren. I allowed the males to approach, and they darted their ſemen as uſual; I then again ſeparated the females, when I

 found

found that the firſt eggs they diſcharged (amounting to ſix, ſeven, or thereabouts) furniſhed me after ſome interval with living newts. But this was not the caſe with others lying higher up in the oviducts, which were brought forth afterwards.

After the amorous encounters were over, I opened a female, and taking the eggs out of the ducts, I put them into water, with the precaution, however, of placing thoſe which lay in the vicinity of the anus in a ſeparate veſſel. Theſe produced young, while the reſt all failed. To theſe proofs, which in my opinion are deciſive, let me add another. The ſpecies of newt, with the golden liſt upon its back (LXXVII), inſtead of bringing forth the ova ſeparate like the other ſpecies produces little cords, ſometimes of the length of two inches, conſiſting of ſeveral dozens of eggs. After the male had careſſed one of theſe females, I opened the abdomen, and took the eggs out of the ducts, which were united together by means of the glue, and formed two cords joining at an angle near the cloaca. I placed theſe cords in water, and marked the end which would have appeared firſt with threads. And the eggs at and near this end produced young, while the others came to nothing.

LXXXV. Though I have proved that theſe eggs are not fecundated, yet it does not follow they do not become ſo in the ſequel. In proportion as thoſe which lie neareſt the anus are expelled, the more remote deſcend and take the place of the former, and are impregnated by new emiſſions of ſemen. The ſame thing happens to the moſt diſtant, ſo ſo that all that arrive at maturity, paſs out of the ovaria, and enter the cavity of the oviducts, are ſooner or later fecundated. I had an irrefragable proof of this, by attending to ſucceſſive diſcharges of ova, which all produced young alike. From what has been already advanced, it is very obvious to infer, that the males do not ſoon abandon the females. From obſervation I have learned, that their amours laſt ſometimes twenty, ſometimes thirty days, and ſometimes ſtill longer, viz. until the females have diſcharged all their mature eggs. For as long as any remain in the oviducts, the males never ceaſe their careſſes, and alternate emiſſions of ſemen, and intervals of reſt (LXXX). I ſaid above *all their mature eggs*, ſince after the amours of the newts are ended, there remain within the ovaria others of very ſmall ſize, and in very great abundance.

LXXXVI. Bomare, in the article already quoted (LXXXII), aſſerts that aquatic newts diſburthen themſelves of the load of their eggs, by laying hold of them with their mouths and feet, and thus promoting the diſcharge from the anus, but that as they are expelled they are glued under the tail. The French writer will I hope pardon me, if I venture to aſſert that no fable can be more extravagant than this. If inſtead of being a copyiſt by profeſſion, as he appears to be from all his publications, he had taken the trouble to obſerve Nature, and in conſequence of this had enquired into the manners of newts, he would have ſeen upon a thouſand occaſions, that the eggs are diſcharged and ſeparated from the anus without the concurrence of the mouth or feet, and that theſe animals never ſhew the leaſt diſpoſition to employ either the one or the other. He would have beſides felt with his hands, that far from following the practice of cray-fiſh, which after having brought forth their ova, keep them attached to the inferior ſurface of the tail, that theſe animals expel them in ſuch a manner, that they all fall to the bottom of the water. Of the infinite number of newts which I have kept in different years, and for different purpoſes, in veſſels full of water, I have never found one individual that carried a ſingle egg adhering to the tail, but when the water in the veſſels was changed, they were always ſeen in an heap at the bottom. I remarked the ſame thing in thoſe that were wandering at pleaſure about the water in the open plains.

And ſince I am ſpeaking of the article *Newt*, compiled by Bomare, let me point out another of the various errors which it contains. He aſſerts, that the cry of the water-newt very nearly reſembles that of the frog (page 35), whereas it is ſufficient to be very ſlightly acquainted with theſe animals, in order to be aſſured that they are quite mute. It is only when they riſe to the ſurface of the water, to expel the old air from their lungs and to inhale freſh, that the obſerver hears a ſort of very low whiſtle, ſcarce perceptible at the diſtance of four paces. But in an author, who ſeems to have inherited inaccuracy and want of diſcernment, as numerous other fabulous tales, with which his Dictionary abounds, may ſerve to ſhew, ſuch errors are leſs unpardonable.

LXXXVII. But let us proceed to the hatching, or rather the evolution of newts, another part of their hiſtory not leſs curious and intereſting than the preceding. Let us then attend to what happens to the eggs after they have been brought forth. Theſe, when put into water, ſink to the bottom; if the weather be warm, a quantity of air-bubbles ſoon appears upon the gluten which includes them; theſe at firſt are very ſmall, but become afterwards larger, and at leaſt ſo large, that the eggs become lighter than water, and ariſe to the ſurface, bringing with them the collection of bubbles ſtill adhering to the gluten: the bubbles then burſt and diſappear, and now the ova fall again to the bottom, and riſe no more, being kept down by the gluten, which faſtens them to the ſpot on which they reſt. If we continue to watch them attentively, we can perceive that their ſhape begins to change. When firſt brought forth, and for one or two days afterwards, they reſemble an elongated ſpherule; the ſpherule now begins to appear ſlightly curved, repreſenting in miniature a kidney, or the teſticle of a cock. The curvature encreaſes, and the bulk in the ſame proportion, but with this additional circumſtance, that one end of the ovum becomes thicker, and the other thinner. In the mean time, it acquires twice its original ſize. And now it appears not to grow in bulk, but only in length; and this becomes every day more apparent to the ſurprize of the obſerver. But his greateſt ſurprize ariſes from ſeeing the egg thus elongated, agitate itſelf at intervals with great briſkneſs, and then continue quiet; and as this happens without any external exciting caufe, the idea of animality neceffarily arifes in the mind, and we incline to believe that the fuppofed egg is a real newt, only in difguife, juft as I have difcovered that the fuppofed eggs of frogs and toads are not eggs, but tadpoles in difguife. This idea continues to be more and more confirmed in the fequel, from obferving by a glafs the felf-moving egg affume the features of a fmall newt, the tail appearing beautiful and perfectly formed, the vertebræ beginning to fhew themfelves as well as the little gills within which the blood circulates, and likewife two lateral protuberances, which the obferver fufpects to be the rudiments of the arms, and the veftiges of the head and muzzle, and laftly the outlines of the eyes lying by the fide of the head, under the appearance of two inconfiderable tumours. Continuing to employ the microfcope, we perceive that the little newt is not now circumfcribed by the gluten, but by a tranfparent circlet more internal, which is nothing elfe than the amnios full of liquor, within which the little animal has its refidence; its colour is alfo remarkable, the inferior part of the body is white, while the upper part is yellowifh, and interfperfed with feveral blackifh ftreaks. N° I. Fig. 16. Pl. III. reprefents a newt's egg in its natural ftate, furrounded by gluten: N° II, III, IV, V. fhew the fame egg ftripped of its gluten, and infenfibly becoming curved and elongated. N° VI, VII. reprefent the body fuppofed to be an egg, but which from its progreffive elongation and evolution, has affumed the real appearance of a fmall newt,

fomewhat

ſomewhat magnified. D. fig. 17. ſhews the ſame circumſcribed by the amnios, which is repreſented by the ſmaller circle, but the whole was obſerved by a microſcope of greater magnifying power.

LXXXVIII. As long as newts remain in the amnios, they are never ſeen extended at full length, but always incurvated in ſuch a manner, that the tail approaches to the head, as may be obſerved in fig. 16. N° VI, VII. and at D. fig. 17. It cannot indeed be otherwiſe; ſince the diameter of the amnios is much ſhorter than the young newt, they muſt of neceſſity be bent. Whilſt they are confined in this priſon, they change their ſituation from time to time, and always with incredible quickneſs, on a ſudden tranſporting the head where the tail lay and reciprocally; and this happens not only when they are ſtruck by any ſubſtance, but even when they are left undiſturbed. Mean while they increaſe in ſize from the food they take within the amnios, but when afterwards that membrane can no longer contain them, they burſt it by repeated efforts, and entirely quit their envelope, and begin to ſwim about the water, by the help of quick vibrations of the tail. I have ſeveral times beheld this exit with pleaſure; it recalled to my memory the exit of butterflies, when they lay aſide the maſk of the chryſalis. In fig. 17, 18, 19. are ſeen three newts juſt out of the envelope; two with their backs preſented, fig. 17 and 19. and one laterally, fig. 18. In the ſame figure we have two others, ſome time emancipated from their confinement, and ſwimming about the water. Theſe two young newts,

newts, as well as the two others that are coming out of their envelopes, fig. 17 and 18. ſhew the prominent rudiments of their fore legs, under the appearance of two little ſtumps. Two of theſe are marked C. C. fig. 17. So the letter A. in the ſame figure, repreſents one of the above-mentioned envelopes. Upon examining ſome, which were no longer receptacles for young newts, they appeared to be externally compoſed of a reſiduum of gluten, which, at its internal ſurface, had become callous, of the extremely fine and tranſparent membrane of the amnios, and of a little of the liquor of that membrane, which was alſo tranſparent and ſlightly viſcid.

LXXXIX. My next object of enquiry was, the time requiſite for theſe animals to paſs from the illuſive form of ovum, to the real one of newt; this I have found to amount to the ſpace of ſeven days, more or leſs. They require three or four more before they come to burſt the amnios and gluten, and to float at liberty in the water.

In general, to *hatch* theſe animals is more difficult, than to hatch the tadpoles of frogs and toads. Though the fetuſes of the newt (falſely ſuppoſed to be eggs) are put into pure water as ſoon as they are brought forth, and this water ſhould have been taken from the very ſpot in which they were depoſited by the females, yet few come to perfection, unleſs we are careful to change the water very often; and even with this precaution, there is always, in like circumſtances, a ſmaller number of young newts than tadpoles evolved.

XC. This difficulty does not occur in their further evolution and growth. Any kind of water, either pit, rain, river, or lake water, will anſwer this purpoſe perfectly well, provided it is pure. If there ſhould happen to be any plant, ſuch, for inſtance, as the marſh lentil, in the water which contains the young newts, they will ſurround and nibble it with their little mouths, ſelecting thoſe parts which are beſt adapted to their conſtitution. During the progreſs of evolution, the arms are unfolded more rapidly than other parts (LXXXVIII), they become pointed, and are bent back towards the poſterior part of the body, as may be ſeen in the two young newts at the bottom of fig. 19. but better in fig. 20. in which the animal appears magnified. The two cones marked C. D. are the two arms; above which, and nearer the head, the gills are ſeen branched, as alſo two other ſmaller bundles of gills, E. F. near the eyes; theſe ſmaller gills appear later than the large ones. The young newts being ſtill kept in water, about a week after their exit from the egg, three other very ſmall cones, not far diſtant from each other, ſhoot from the ends of the two former; theſe cones are ſoon perceived to be fingers belonging to the little hands. The animals now begin to employ theſe fingers, hands, and arms. In like manner as the anterior limbs make their firſt appearance in the ſhape of two cones, ſo alſo do the poſterior, which commonly are firſt perceived, fifteen and ſometimes twenty days after the animals have quitted their envelope. Nature likewiſe obſerves the ſame progreſs in producing the toes as the fingers; and

and now the newt is able to walk, either along the bottom of the water, or on dry ground. It is well known, that the tadpoles, both of frogs and toads, loſe their gills a few days after their birth. Young newts, on the contrary, retain them long, and I have even obſerved them in Auguſt.

XCI. There ſtill remains an important enquiry relative to theſe animals, the ſame which has been already made concerning frogs and toads. At what period may thoſe roundiſh bodies, commonly called the eggs of newts, be properly termed true fetuſes? Is it when they are upon the point of being expelled from the body of the female, and conſequently have been already bathed by the ſemen? or ſtill ſooner, when they lie in the higheſt part of the oviducts, where we are certain that the ſemen could not arrive? I flatter myſelf, that I have the moſt direct proofs that theſe little bodies are real animals, even when they are at the top of the oviducts; although when taken out of the body of the female, they are not developed, but come to nothing, for want of the neceſſary condition of being fecundated by means of ſemen. Theſe proofs are deduced from the identity of the impregnated and unimpregnated corpuſcles: and I hope the reader will be fully convinced of this identity, when I aſſure him, that I have ſubjected each to the ſame minute and rigorous analyſis, that was employed upon the fetuſes of frogs and toads, and which is particularly deſcribed in the LXXVIIIth paragagh; nor could I diſcover the ſmalleſt difference, either in the internal or external

parts,

parts, between the corpuſcles bedewed with ſemen, which were evolved and became newts, and others not touched by that liquor which were ſpoiled. I therefore conclude, that in theſe animals alſo, the fetuſes exiſt in the females before they are impregnated by the male.

XCII. I have ſpoken of the two ſorts of newts deſcribed in paragraph LXXVII, and which have been hitherto the objects of my enquiries, as if there had been only one, having obtained from both the ſame reſults in every thing that reſpects generation. I will moreover add, that theſe reſults have been the ſame in another ſpecies of this animal, little more than an inch and half in length, and about two lines in thickneſs, of which the colour is a cineritious ground, interſperſed with black ſpots; my obſervations upon this ſpecies were made three years after thoſe which are related in the preſent chapter.

I have hitherto related my remarks concerning the generation of frogs, toads, and newts, in ſuch a manner, that it does not always appear, whether I have made ſuch a number of obſervations and experiments as is neceſſary, in order to obtain ſafe and conſtant reſults. Now the reader may be aſſured, that I have done this, not becauſe I was not in poſſeſſion with a ſufficient number of facts, but in order to avoid creating diſguſt by too frequent repetitions. I can alſo aſſure him, that every fact has been ſeen and examined a great number of times, for I have been taught by daily experience, that in natural

hiſtory,

hiſtory, truth can only be attained by the conſtant ſucceſs of repeated experiments.

CHAP. VI.

REFLECTIONS.

XCIII. MY principal intention in the five preceding chapters, was to relate facts unmingled with reflections. I would now deduce conſequences, and lay before my readers ſuch a train of reaſoning, as may contribute to the illuſtration of the ſubject, for this is the moſt important part of natural philoſophy.

I. It is well known, that almoſt all animals except man, have a ſtated ſeaſon for the propagation of their ſpecies. Thus the female cat receives the male in September, January, and May. The ſhe-wolf and fox in January; the doe in September and October. The ſpring and ſummer are the ſeaſons appointed for the amours of birds, and many ſpecies of fiſhes. The immenſe tribe of inſects have likewiſe a determinate time for perpetuating their kind; this is the fine part of the year, and particularly autumn and ſpring. The laſt mentioned claſs of beings is ſubject to a variation that is not obſerved in the others. Unuſual warmth or cold does not retard or forward the conjunction of birds or quadrupeds;

rupeds; but a late ſpring delays the amours of inſects, and an early one forwards them. Thus it is obſerved, that in the ſame country, the inſects on the mountains are later than in the plains.

XCIV. The variations of heat and cold have the ſame effect upon the amphibious animals, that have been the ſubject of my enquiries, as upon inſects. They too have, as we have ſeen, a fixed time for their amours, but it varies according to the warmth or coldneſs of the ſeaſon. If we compare my obſervations on frogs and toads with thoſe of Roeſel, it will appear, that this order of animals begins earlier to propagate the ſpecies in the mild climate of Italy, than in the rigorous temperature of Germany. In his hiſtory, he ſpeaks of a very ſmall ſort of aquatic toad, which, from the beautiful flame-coloured ſpots that adorn the belly, he calls Bufo igneus, as copulating near Nurimberg in June. This ſpecies is not found in the plains of Italy, at leaſt not in the Modaneſe, the Milaneſe, or the country round Pavia. I have ſometimes found it on the hills round Modena, the male embracing the female in May, but I had not an opportunity of making my obſervations with ſufficient accuracy. Afterwards, in my journey to Geneva and Switzerland, in the ſummer of 1779, I ſaw a great number of theſe toads. In the courſe of my excuſions with the amiable and learned Mr. Senebier, on the eminences that almoſt ſurround that illuſtrious and cultivated city, I remarked, that there was not a lake or ditch that did not afford reception to great numbers of theſe animals; they were generally coupled,

coupled, though it was then the end of July. I have moreover found them in Auguſt, in many waters in the vicinity of Berne, Baſil, Zurich, and Lucern, and their amours were then but juſt finiſhed, as appeared from the little tadpoles that had lately quitted their mucus, and were ſwimming about. Theſe animals then copulate much earlier in Italy, than either in Germany or Switzerland. This is not matter of ſurprize, for the Swiſs cantons are generally known to be the higheſt part of Europe, and by conſequence muſt be cold, as I perceived among other proofs, which cannot be properly introduced here, by the ripening of the corn, which is a month later than in the plains of Italy, and by thoſe trees, which with us will not grow at all, or elſe only in the higheſt ſituations, thriving here wonderfully in the loweſt vallies, ſuch for inſtance as the larch, the beech, the fir, &c.

I have obſerved in the tree-frog, and fetid terreſtrial toad, in the river at Geneva, juſt the reverſe of what I had ſeen in the Bufo aureus in Switzerland. In March 1780, I remarked that the two former ſpecies had made their appearance, and were coupled in the ponds and other reſervoirs of fine gardens near that city, whereas in Auſtrian Lombardy, they had not yet quitted their ſubterraneous abodes. The reaſon why frogs, toads, newts, and the numberleſs tribe of inſects, require a certain degree of atmoſpherical warmth, is, I ſhould think, becauſe the latter have an internal ſource of heat, which animates them, even in the ſevereſt cold, whereas the former are almoſt entirely deſtitute of any ſuch

ſource.

ſource. As therefore the exerciſe of their functions depends on the heat of the atmoſphere, their amours will alſo depend upon this cauſe, and will of courſe be later in cold than hot climates, and in both will vary with the ſeaſon.

XCV. II. It has been ſhewn in the preceding chapters, that the round bodies produced during the ſeaſon of generation by the female frogs, toads, and newts, that fell under my inſpection, are not, as it has been univerſally believed, eggs, but real fetuſes; for they are never incloſed in membranous or cruſtaceous envelopes, like animals that come from eggs, but are quite naked; nor do they ever leave any ſhell or membrane behind them, after they have been impregnated, except the amnios, which is the caſe with all viviparous animals. It follows, that theſe ſpecies ought to be removed from the claſs of oviparous animals, to which they have been referred by naturaliſts and nomenclators, and placed among the viviparous. There is a circumſtance here that deſerves to be noticed. All viviparous animals have this in common, that their fetuſes are at birth full formed, and retain the lineaments which they then have through their whole life: they are only more unfolded. We are further certain, that they have long before birth the form of the ſpecies, as is evident from human abortions, as well as thoſe of beaſts. In like manner, animals that come from eggs are formed, not only when they are hatched, but long before, as we ſee in the eggs of birds, various reptiles, crocodiles, &c. If the eggs are broken and examined, we ſhall perceive the fetuſes more or less advanced, provided they have been fecundated and set to hatch. I have made the same observation on the eggs of insects; when I found they were nearly hatched, I have frequently opened the pellicle, and discovered the embryo formed, and endowed with the power of motion. On the contrary, the fetuses of the amphibious animals, that have been the subject of my researches, are quite shapeless at the time of exclusion, and have only the appearance of globules; it is not till afterwards that the limbs begin to appear, and they assume the lineaments of the species. Now I think that upon reflection, I can assign the physical cause of this striking difference. The fetuses of other animals have, indeed, at the time of birth, the characteristic form of the species, but they do not acquire it for some time after fecundation. They are at first shapeless, as we see in birds in the egg, which, before they assume their true figure, must undergo the most surprizing changes, as has been shewn by Haller, and before him by Malpighi. Fetuses then, in general, are not perfectly formed till some time after fecundation. Now in the amphibious animals of which I have been treating, impregnation does not take place till the fetus is excluded. It is, therefore, no wonder that they are shapeless; it is indeed to be expected, that they should take the form of tadpoles after they are brought forth. And we might conclude, that the globular figure which these animals have, as long as they lie in the ovaria, might perhaps be the universal model for unimpregnated germs, if they were not the

production

production of infinite wiſdom, and therefore probably infinitely varied.

XCVI. III. Though theſe animals are properly denominated amphibious, ſince they are capable of living both upon dry ground and in water, yet, if we except the common frog and the newt, they generally live out of the water, and only repair to it in order to propagate the ſpecies; and as if conſcious that their fetuſes, if brought forth upon land would irretrievably periſh, they invariably depoſit them in water, the only element where they can be unfolded and grow. They do not, however, place them in any water that may happen to be near: they are never to be ſeen in torrents or rapid rivers, but always in ditches, lakes, ponds, where the water ſtagnates or runs but ſlowly. The reaſon of this is very obvious. A rapid current would carry the fetuſes away, for they are ſpecifically lighter than water, and by continually ſhaking and daſhing them againſt the banks and other obſtacles, would deſtroy the greater part. The ſame danger does not ſubſiſt in ſtagnant waters. Beſides the tadpoles here find their food, which chiefly conſiſts of water lentil, a plant that does not grow in quick ſtreams.

Theſe animals then, as well as others, carefully provide for the continuation of the ſpecies. In inſects in particular, theſe proviſions are an inexhauſtible ſubject for admiration. The curious obſerver of theſe minute beings, is conſtantly ſurprized by the care they take to depoſit their eggs, where the the young are ſure to meet with proper food. An immenſe number of both nocturnal and diurnal butterflies and moths, come from grubs that live upon various herbaceous and woody plants. They never fail to depoſit their offspring preciſely upon theſe plants. There is not the leaſt danger leſt they ſhould miſtake one vegetable for another. That which comes from the grub of the oak never lays its eggs on the elm, and reciprocally. The ſame may be ſaid of other plants; for ſhould ſuch a miſtake happen, the grubs would die for want of fit nouriſhment. Another claſs of inſects, the numberleſs and various tribe of flies, affords equal cauſe of admiration: they all depoſit their eggs in certain ſtated places; ſome in the pith or within the bark of trees, or upon the leaves, or within the empalement of flowers; ſome in the ground or in water; ſome upon dunghills, in ſepulchres or dead bodies, and others, in ſhort, within the body or upon the ſkin of living animals.

Equal induſtry and ſagacity is conſpicuous in the race of cantharides, beetles, &c. And to come to animals, more nearly reſembling thoſe concerning which we are enquiring, many fiſhes, belonging both to ſalt and freſh water, conceal their eggs in places where the water is ſhallow, and conſequently more warmed by the rays of the ſun; hence the young fry is ſooner produced. Beſides theſe places generally afford more aquatic inſects, the food moſt agreeable to fiſhes. Turtles, which are themſelves amphibious, furniſh an example ſtill more cloſely connected with the ſubject of theſe diſſertations, excepting that they repair to dry ground, and conceal their eggs in the ſand.

XCVII. IV. In the four firſt chapters, I mentioned the cloſe embraces of the male frogs and toads. This phænomenon has given riſe to diſcordant opinions. Valliſneri thinks, that the compreſſion contributes to ſeparate the eggs from the ovaria, and to facilitate their entrance into the oviducts (*a*). Swammerdam, on the contrary, ſuppoſes that it is more likely to prevent, than forward their paſſage through the oviducts, as it may cloſe their orifices. Hence he thinks, that the male does not begin his embraces, till the eggs have paſſed through thoſe canals (*b*). But this can only be determined, by opening ſome females at the commencement of their amours. In ſome ſpecies, indeed, bare inſpection will ſhew, that the preſſure made by the fore-feet of the male, cannot contribute to force the eggs out of the ovaria; for they do not come forward upon the breaſt, to which the chief part of the ovaria lies oppoſite, but preſs againſt the lower extremity of the abdomen, as I have myſelf obſerved in the Bufo igneus of Roeſel, and as indeed is evident from his figure. I kept two females from the male, but this did not prevent them from diſcharging their eggs. Hence it is more than probable, that this would alſo happen in the ſpecies, denominated by that naturaliſt, Bufo aquaticus allium redolens; for the male does not preſs againſt the breaſt, but like the igneous toad againſt the abdomen of the female (*c*). There ſeems, however, ſome ground for believing, that the em-

(*a*) L. C. (*b*) Bibl. Nat. (*c*) See Roeſel's figure.

braces of the male may contribute to the exclusion of the eggs, in thoſe ſpecies in which he claſps the female round the breaſt; as in thoſe frogs and toads, of which I have deſcribed the mode of generation in the four firſt chapters. If the females of the aquatic frog be kept ſeparate from the male, I have found, that they do not diſcharge their fetuſes (VII). Here then we muſt conclude, that the embraces of the male influence this function.

I know not how this influence can be exerted, except by the preſſure made againſt the breaſt, by which the fetuſes are forced out of the ovaria, and get into the mouths of the oviducts. But in the tree-frog, notwithſtanding the preſſure is made againſt the breaſt, it does not contribute to the deſcent of the fetuſes, ſince they get into the uterus before the male begins his embraces (XXVI). When I wrote the hiſtory of the terreſtrial toad, with red eyes and dorſal tubercles, I knew not whether the female could bring forth without the male; and I only ſaid, that his embraces begin before the fetuſes quit the ovaria (XLII). I afterwards kept two females in a veſſel full of water, which, without the approaches of the male, diſcharged their long and viſcid cords, but the fetuſes all periſhed for want of fecundation. If we now compare the facts related in this paragraph, we ſhall find, that Swammerdam's opinion is not univerſally true; for although the embraces of the male are poſterior to the deſcent of the fœtuſes into the uterus in the tree-frog, yet this does not happen in the aquatic frog and in toads. Secondly, it will appear, that

that the ſtrong compreſſion of the male is very far from being always, as Valliſneri imagined, the cauſe of the ſeparation of the fetuſes from the ovaria. We cannot therefore on this, any more than on numberleſs other occaſions, lay down any general rule, but muſt be attentive to the variation of Nature, in the endleſs multiplicity of her operations.

XCVIII. V. I ſhall, perhaps, be aſked to point out the cauſe of the male's perſeverance in his embraces, which in ſome cold countries laſt above forty days, as Swammerdam obſerved in Holland. It may alſo be enquired, why theſe animals, during their amours, not only abſtain from food, but diſregard their own ſafety, for at this time they will not make any efforts to avoid being taken.

I can aſſign no other cauſe, but that phyſical neceſſity which compels the individuals of different ſexes to approach each other at this ſeaſon. Under the dominion of this influence, which, in theſe amphibious creatures, as in other animals, is probably more ſtrongly felt by the male than the female, they go in queſt of each other and copulate. The copulation muſt laſt till their mutual neceſſities are ſatisfied; this neceſſity, in the female, conſiſts in the expulſion of the fetuſes, and in the male, in the emiſſion of ſemen; for the amorous impetus is occaſioned and exalted by this fluid, with which the ſpermatic veſſels are gradually filled during their embraces. Their pertinacity may have indeed another origin; they may fear, leſt if they ſhould quit their female, ſhe ſhould be occupied by another male; we know that animals are not exempt from jealoufy; and I have had occafion to remark, that it is particularly vehement in thofe of which we are fpeaking. Their amorous ardour, which, at this feafon, feems to be their only feeling, may render them infenfible to the call of hunger and to danger. The long adhefion of the male to the female, and the neglect of themfelves, is not peculiar to frogs and toads, but extends to various other animals, as we every day obferve in infects, and in fome animals of confiderable fize; as in the turtle, of which naturalifts have obferved, that the male and female continue attached to each other for feveral days; and in this fituation, the fifhermen eafily take them together.

XCIX. VI. This blind ardour of the male, has furnifhed me with an opportunity of attempting fome uncommon experiments, with an account of which, I hope my readers will not be difpleafed. In paragraph XLI it is faid, that if a couple of the fpecies of toad with red eyes and dorfal tubercles, happened to efcape out of my veffels, when coupled, the male did not quit the female, notwithftanding fhe got to dry ground. I was further defirous of knowing, whether a feparation would take place in confequence of violence. I therefore fufpended a male, by a thread tied to one of his hind feet, for a quarter of an hour; he would not, however, loofe his hold, notwithftanding the long continuance of this violent attitude, and the weight of the female, which was fo much larger than himfelf. I next pricked him with a needle in the hind legs, the thighs, the back, fides and head, till the blood iffued out at every

punćture,

puncture. He would move and writhe, and ſtretch out and contract his body, but did not quit his hold. I afterwards cut the body in various parts with ſciſſars; but this expedient was ſtill ineffectual; nor would he forſake his ſituation, when I cut away ſmall pieces of fleſh; I even amputated a thigh without effect, and it was not till after thirteen hours, that the male, after ſo much torture, parted from the female, and expired the ſame inſtant.

I put another male to this female, and as ſoon as he had claſped her cloſely, cut off both his thighs without cauſing a ſeparation: and what is more ſurprizing, the female began to diſcharge the cords three hours afterwards, and the male, with his blood flowing all the time, continue to impregnate them with ſemen, till the whole was diſcharged. The impregnation was effectual, and, as uſual, the greater part of the fetuſes were evolved.

I ſeparated the male of another couple by force, and then cut off his two thighs. He was left with the female, that it might be ſeen, whether he would reſume his employment; an event which actually took place, but he died before the excluſion of the cords.

My laſt experiment conſiſted in cutting off the fore-feet of a male, and putting him to a female. Now the fore-feet enable the male to claſp the female ſo cloſely. The ſubject of my experiment inſtantly leaped upon her back, as if he had ſuffered nothing, and maintained his poſition, embracing her with his bloody ſtumps, till he had ſprinkled the cords with ſeminal fluid.

C.

C. I will not enter into the particulars of other like experiments, that were made upon the fetid terreſtrial toad and frogs. I will only obſerve, that they coincided with thoſe I have related: the amputation of the limbs neither preventing the embraces nor fecundation. Nay, even the decapitation of a frog, did not prevent either the one or the other. It is well known, that theſe animals are ſo tenacious of life, that this operation does not take it away immediately. That of which I am ſpeaking was thrown into convulſions, but neither the fore-feet nor legs quitted the breaſt of the female, which brought forth her fetuſes in an hour and three quarters, and I was an eye-witneſs of the male's beſprinkling them with ſemen; that they were fecundated, there can be no doubt, ſince they came to life at the uſual time. As ſoon as he performed this operation, he deſerted his ſituation, and died four hours afterwards.

The toad mentioned in paragraph XCIII, was alſo the victim of an experiment of the ſame ſort. In Auguſt 1779, at Genthod, the delightful villa of Mr. Bonnet, I cut off both the thighs of a male of that ſpecies, while it was embracing the female; but without effecting a ſeparation, which did not take place till many hours afterwards, a little before the death of the animal. Beſides Mr. Bonnet, Mr. John Trembley was preſent at this experiment. I was aſked, whether this pertinacity of the male was the effect of ſtupidity, inſenſibility, or amorous ardour. Though this appeared to be one of thoſe queſtions, of which the determination requires, that one

ſhould

ſhould enter into an animal without at the ſame time becoming one; yet I heſitated not to ſay, that I thought, as I now do, that this perſeverance was leſs the effect of obtuſeneſs of feeling, than vehemence of paſſion, which, as we have ſeen, renders them inſenſible to the call of hunger, and careleſs of their ſafety (XCVIII). Such a degree of ſtupidity cannot, I think, conſiſt with the great irritability of theſe animals, and the ſigns they ſhew of keen feelings, whenever they are wounded, or have their limbs cut off during their amours.

CI. VII. Swammerdam, in his account of the generation of frogs, ſuppoſes, that the eggs do not immediately paſs from the ovaria into the oviducts, but that they paſs firſt through the abdomen. He reſts his opinion upon the inſtance of a frog, in which he found the eggs partly in the ovaria, and partly in the abdomen, beſides thoſe which were already in the oviducts and uterus. Roeſel, ſpeaking of the dark-brown terreſtrial frog, ſays, that he has found many eggs in the abdomen, without, however, adopting or rejecting the opinion of Swammerdam. Of theſe eggs, or more properly, of theſe tadpoles, lying looſe in the cavity of the abdomen, I have already ſpoken (LXVII); but if I may confeſs what I think, I have no inclination to accede to the opinion of the Dutch naturaliſt. I ſhould imagine, that if the fetuſes were to paſs through the cavity of the abdomen before they entered the oviducts, they would frequently be found there in females during copulation, ſince they are ſo often found in the oviducts and uterus. Yet the

the contrary is true. I wish to avoid the charge of ostentation, when I observe, that having kept an account of the number of frogs and toads, which I have opened at the time of their amours, for this and the following dissertation, I find that it amounts to 2027; nor did I ever find the fetuses in the cavity of the abdomen, except on three occasions, of which two are mentioned in paragragh LXVII, and the other in CXXII. I am much more disposed to believe, that fetuses pass immediately from the ovaria to the oviducts. Hence I think, that if ever they get into the cavity of the abdomen or thorax, it proceeds from their never having got into the oviducts, or from some laceration, as is intimated in paragraph LXVII. Let the impartial reader determine.

CII. VIII. The fetuses, after having traversed the long and tortuous canal of the oviducts, are all collected in the uterus, whence they pass into the rectum, and then out at the vent. The discharge, in one species of toad, is aided by the male. Placed as usual upon the back of the female, and clasping her with his fore-feet, he waits impatiently for the expulsion of the cord (in this species there is only one cord); at the instant of its appearance, he lays hold of the end with his toes, and draws out a piece of the cord; this manœuvre is repeated till the whole cord is extracted. So intent is the male upon his employment, that he may be taken and placed upon the hand. And though this may cause some interruption, yet he will soon resume his task with equal ardour. This information is owing to a casual observation of Mr. Demours,

Demours, on the ſmall ſpecies of terreſtrial toad, as he denominates the animal, he ſaw nothing from which he could collect, that the male bedews the eggs with ſemen, while he is employed in extracting them (*a*).

It is to be lamented, that the French obſerver did not particularly deſcribe the ſpecies. It is certainly different from either of thoſe, of which I have treated; ſince I could never obſerve, that the male aſſiſted in bringing forth the young, while it was eaſy to ſee, that he bedewed them with ſemen. Roeſel, who mentions this fact incidentally, adds, that he never obſerved any appearance that could be compared with it: it might likewiſe have been expected, from the accuracy of the obſerver, that he ſhould have taken notice whether the eggs were productive, ſince we might have hence collected, whether they are impregnated within the body of the female. It were alſo to be wiſhed, that ſo intereſting an obſervation ſhould have been repeated. In ſhort, the fact being ſolitary, and happening unexpectedly, ſhould have been confirmed, and the narration ought rather to awaken than ſatisfy the curioſity of the philoſopher.

CIII. IX. Daily experience ſhews, that in an immenſe number of animals, fecundation takes place within the body of the female. It might perhaps be thought, that we are warranted to conclude, from analogy, that this is an univerſal law of Nature. And it has accordingly been admitted as ſuch by

(*a*) The reader will find this very curious obſervation related at full length in the Appendix. T.

vulgar

vulgar reaſoners. But as on many other occaſions, when the law was ſuppoſed to admit no exception, ſo on this, analogical arguments have been found to diſagree with experiment. Swammerdam firſt ſhewed, that impregnation is effected without the body of the female in one ſpecies of frog; and Roeſel extended this diſcovery to another amphibious animal of a ſimilar kind. I have had the ſatisfaction to diſcover this external fecundation in other ſpecies of frogs and toads, and have, moreover, clearly beheld the fecundating liquid iſſuing from the male, and falling on the fetuſes, after they were expelled from the uterus of the female (Chap. I, II, III, IV).

CIV. But we know, that beſides the ſpecies, in which it has been found that impregnation is external, there are many others included under the genus of frogs, as well as that of toads, both European and foreign. We ſhall be convinced of this, by opening any modern nomenclator, as Linnæus for inſtance. What then are we to conclude, concerning the mode of fecundation in them? From theſe conjectures we may be willing to believe, that it is the ſame in the ſpecies that have not been examined. But we cannot be certain without experiments. Among thoſe, which in this reſpect require to be examined by the naturaliſt, the famous toad of Surinam, denominated by the natives *Pipa* or *Pipal* (*a*), ſo remarkable for its pro-

(*a*) Pipa (Rana) Digitis anticis muticis quadridentatis, poſticis unguiculatis. Linn. S. N. T. 1.

perty

perty of bringing forth the young at the back, would, in my opinion, deſerve the preference.

The celebrated Merian, who, with courage truly heroic, went from Holland to America, towards the cloſe of the laſt century, to obſerve the inſects of that unwholeſome climate, firſt made us acquainted with this animal. It was afterwards examined by the acuteſt naturaliſts, by Ruyſch, Folkes, and Baker, and found exactly to agree with the deſcription left by that illuſtrious lady. Its back has cavities or cells, each containing a young toad. I had myſelf the ſatisfaction of ſeeing this rare and indeed ſingular phænomenon, in the public inſtitute of Bologna, and to ſtill greater advantage laſt year at Geneva, in company with my reſpectable friends Bonnet, Senebier, Abraham and John Trembley. Nay, the poſſeſſor, who had many years kept it in ſpirits of wine, obligingly afforded Mr. Bonnet and myſelf an opportunity of making a number of obſervations upon it; and above all, to examine at leiſure the dorſal cells, which were very numerous, and of which each contained a fetus. Theſe obſervations, together with others, which my illuſtrious colleague made after my departure from Geneva, may be found in a Memoir printed in Rozier's Journal, which at once ſhews the learning and good ſenſe of the author, as well as his impartiality with reſpect to his own opinions. Having denied, in his *Corps organisès*, the exiſtence of the cells, on the authority of a celebrated profeſſor at Leyden (*a*), he not only confeſſes in the Memoir,

(*a*) Mr. Allamand.

5

moir, they are not to be found, but deſcribes them in a manner ſo particular, as to remove all doubts concerning their exiſtence. He concludes with propoſing ſeveral queſtions, calculated to illuſtrate the natural hiſtory of this wonderful native of Surinam, which is yet dark and imperfect. It is eaſy to ſuppoſe, that the Geneveſe philoſopher would not overlook what relates to the manner of impregnation. But we cannot hope for the ſolution of this, any more than the other queſtions, except upon the ſpot where the pipa lives, and propagates its ſpecies, if we cannot naturalize it, as Valliſneri naturalized the cameleon of Africa.

But beſides frogs and toads, it is ſuppoſed, that fecundation is external in ſcaly fiſhes. When the female has laid her eggs, the male is ſaid to go in queſt of them, and ſprinkle them with ſemen. There is then no real copulation between theſe animals, though the male approaches the female at the ſeaſon of their amours, and is ſometimes ſeen to rub his belly againſt that of his mate; this appearance takes place, becauſe the male impregnates the eggs at the inſtant they fall from the female. This is the opinion of Buffon (*a*); and the manner in which he delivers it, would lead us to ſuſpect, that he had the moſt certain proofs of it, though he has in truth no better foundation for it, than the notion which was generally prevalent till the time of Swammerdam (*b*), that the cetaceous claſs impregnates the eggs without copulation. This notion was not, I think,

(*a*) Nat. Hiſt. T. 2. p. 313. (*b*) Bibl. Nat.

ſupported

ſupported by any obſervation upon which reliance could be placed. Hence it is no wonder that other naturaliſts, and among the reſt the illuſtrious Haller, ſhould incline to believe, that fiſhes really copulate for ſeveral reaſons, which cannot properly be recited here, but may be ſeen in his great work. Though theſe reaſons have far greater weight than the bare aſſertion of Buffon, yet I cannot think them deciſive, as they are deſtitute of facts, by which alone the problem can be diſſolved.

The mode of fecundation in fiſhes is very extraordinary, according to Linnæus; he ſuppoſes, that the female purſues the male while he is emitting the ſemen, and devours it, and thus is impregnated (*a*). In the time of Valliſneri, there lived at Rome a phyſician, who taught that pigeons, ſparrows, and many other animals, were fecundated by the mouth (*b*). Both theſe opinions are palpably falſe. Female fiſhes have, indeed, been obſerved to ſwallow the ſemen, not becauſe it then ſerves to impregnate them, but ſimply for food. The male devours it with equal greedineſs for the ſame reaſon. The ſame obſervations are applicable to the eggs.

CV. Hence it appears, that we know not certainly the mode of fecundation in fiſhes. The element they inhabit, is ſo much more difficult of acceſs than the land and air, that we cannot wonder the natural hiſtory of fiſhes is ſo little advanced. Having often meditated upon this ſubject, which is yet among the myſteries of nature, I have been ſtruck by a

(*a*) Sponſ. Plant. (*b*) Valliſn. op. in fogl. T. 3.

thought, which my other occupations have not yet permitted me to bring to the teſt of experiment. I willingly lay it before my readers. The golden fiſhes of China (*a*) are now very common. Italy in particular abounds with them, there being ſcarce a pond that is not ſupplied with them. The beauty of their colours induces many to keep them in veſſels in their apartments. The amours of theſe fiſhes happen ſeveral times a year, and as they have little timidity, they do not deſiſt from their employment when the obſerver ſtands cloſe to them. The reader is by this time in poſſeſſion of my ideas. Let theſe fiſhes be carefully watched when they are propagating their ſpecies, and we ſhall ſoon know, whether they really copulate, or whether the male darts his ſemen upon the eggs after they are diſcharged, or, in ſhort, whether fecundation takes place in any other way. If it is external, the aſperſion of the ſemen cannot eſcape the attentive obſerver, as it is of a turbid colour. I will not ſtay to point out the means of aſcertaining whether the eggs are fecundated, and in what manner, as the reader may gather them from the preceding obſervations on the generation of amphibious animals.

CVI. Recent obſervations ſeem to have eſtabliſhed the certainty of external fecundation in bees. If the ingenious obſervations (*b*) of Mr. Debraw may be relied upon, the eggs of this induſtrious inſect are impregnated af-

(*a*) Cyprinus auratus, pinna ani gemina cauda tranſverſa bifurca. Linn. S. N. T. 1.

(*b*) Philoſoph. Tranſ. Vol. LXXII.

ter

ter the queen has difcharged them. She, as it is well known, depofits them in the cells. The Englifh naturalift obferved, that when they are accompanied with a whitifh liquor, which the male voids from the pofterior part of his body, they never fail to be productive; but when this liquor is not prefent, they are fure to perifh. The author kept an hive without drones. The queen laid her eggs, as ufual, but as they were not impregnated with the white liquor of the male, they produced no young.

The experiment was varied in the following manner. An unimpregnated hive was divided into two parts, of which one was fet under a glafs-bell; in this was a queen, together with working bees, but without males, the eggs were not productive; but the contrary happened in the other part of the hive, which was likewife fet under a glafs-bell, and contained, befide a queen and common bees, a quantity of males. And the eggs were impregnated by the white liquor which the males void in the cells containing eggs. The diffection undertaken by the Englifh obferver proves, that this liquor is the true femen; for he found it in thofe veffels of the male, which are univerfally fuppofed to be appropriated to the reception of the feminal fluid.

From this it appears, that the two naturalifts, who have written fo well concerning bees, have fallen into error. Swammerdam thinks, that exhalations from the males are abforbed by the female, and ferve to fecundate the eggs; while Reaumur has been led by deceitful appearances to fuppofe, that thefe insects perpetuate their kind by real copulation. Moreover, the suspicion entertained by the celebrated Maraldi, is completely verified; this writer, in his observations on bees, conjectures, that the eggs are impregnated after they are laid, by the whitish matter above described; he, however, was not diligent to enquire further.

From what has been said on external fecundation, it appears, that the number of animals, in which we are certain that this mode takes place, is very inconsiderable. We may presume, that the industry of observers will increase it, for many other discoveries have been extended to a multitude of subjects, though they at first seemed to be confined to a single species.

CVII. X. Fecundation in newts is accompanied with circumstances, that are not common to other animals, and deserve to be considered. The fetuses (for the elongated bodies, supposed to be, eggs are only young newts not yet unfolded LXXXVII), are impregnated within the body of the female. But the male does not introduce into her any part that characterizes the sex, for, in truth, he has not any such part; he only darts his semen into the water; the semen gets into the anus of the female prepared to receive it, and thus she is fecundated. And here occurs another singularity that deserves to be noticed. According to the general opinion of the best anatomists and physiologists, the ovaria are the seat of fecundation; but this cannot be the case in the newt. When the seminal fluid gets to the inferior orifice of the oviducts, it cannot possibly pass any further, for

for its paſſage is ſtopped by the fetuſes, which now occupy more or leſs of theſe canals, and are generally thickeſt at the mouth. The ſeminal fluid then ſtops here, and moiſtens the fetuſes as they come forth. Theſe are the only ones that are fecundated; but they are ſucceeded by others, which are in like manner fecundated by new emiſſions of ſemen, and ſo on till all are impregnated. I ſuppoſe the reader to recollect paragraghs LXXX, LXXXI, LXXXII. LXXXIV, LXXXV. upon which theſe deductions are founded.

CVIII. XI. Though the chief object of this diſſertation is generation, yet I have hitherto conſidered it in a few animals only. I ſhall now extend my ideas, and make uſe of the data which my experiments afford, as principles that will prevent me from falling into error in this intricate enquiry. The moſt celebrated ſyſtems concerning generation may be reduced to two: the one attempts to explain the formation of animated beings mechanically; the other ſuppoſes, that they are already formed and pre-exiſting, and that the act of fecundation only unfolds them, and renders them viſible. Thoſe who favour this ſecond ſyſtem, are divided into two parties; ſome naturaliſts being of opinion, that the fetus pre-exiſts in the female, and others in the male. It is generally known, what efforts the eloquent Buffon has made to bring the former ſyſtem (which is called the ſyſtem of Epigeneſis, and is of more ancient date) into repute, by means of his famous *organic molecules*. The powerful oppoſition that has been made to this ſyſtem, is alſo well known. It has been ably refuted by the great Haller, not only in his phyſiology, but alſo in a ſeparate publication, entituled *Reflections on the Syſtem of Generation of Mr. Buffon.* The arguments adduced by Mr. Bonnet in his Corps organiſés, are not leſs cogent. Obſerving, however, that the objections of both Haller and Bonnet, though of great moment, are not direct, ſince theſe writers have not enquired into the exiſtence of the organic molecules, which conſtitute the baſe of Mr. Buffon's edifice, I conceived it neceſſary to enter into an examination of them, and have found that this ſyſtem, like all his other favourite hypotheſes, is the product of his fervid fancy, which ſo repreſents ſhadows, as to make them appear like realities to thoſe, who have not a conſiderable ſhare of diſcernment. I flatter myſelf, that my Eſſays (*a*) prove the truth of my aſſertions.

The preceding obſervations on amphibious animals, furniſh another irrefragable argument againſt the French naturaliſt. He thinks that the fetus does not exiſt before fecundation, but is formed during this act, for then the organic molecules, which are, according to him, the eſſence of the ſemen of the male and female, meet and combine in the uterus, and by virtue of certain relations, are modelled into an organized body. But my obſervations on frogs, toads, and newts, are diametrically repugnant to this imaginary theory. They prove, that the fetus exiſts in the female long before impregnation (XVIII,

(*a*) Opuſcoli di fiſica animale e vegetabile. Modena 1773.

XIX.

XIX. XXX. LIV, LV, LVI, LVII. LXXII. XCI). Now this is probably the caſe in other animals. My experiments, indeed, were all made upon animals of cold blood; and this circumſtance may afford room to doubt, whether the concluſion is to be extended to thoſe of warm temperature. But all ſuſpicion muſt be removed, now the ſame obſervations have been made upon this claſs. I allude to this luminous diſcovery of Haller, who has ſhewn, that in birds the young exiſts in the female before fecundation; but as this diſcovery is very generally known, it will be unneceſſary to relate it here. As then we have, both in the claſs of cold and that of hot animals, inſtances of the pre-exiſtence of the fetus, I can ſee no reaſon, why we ſhould not apply the obſervation to the reſt. We have at leaſt, till the contrary ſhall be proved, good grounds for believing that this is the caſe.

CIX. But theſe obſervations lead to other conſequences. I have remarked, that thoſe who admit the pre-exiſtence of the fetus, are divided into two parties, of which one believes, that they lie in the female, the other in the male (CVIII). According to the latter, the fetuſes are the worms that float in the ſemen, and paſs, during coition, from the male into the female. But the falſity of this opinion is now obvious. In paragraph VIII. I obſerved, that after the oviform corpuſcles, or the mature fetuſes of the green aquatic frog, have deſcended into the uterus, the ovaria contain others of a ſmaller ſize, which ſerve the year following to continue the ſpecies. The like remark was made (LXVI and LXXXV), concerning the fetid terreſtrial toad and water-newt. I may now add, that I have diſcovered the ſame thing in the other amphibious animals mentioned in this work; ſo that we may ſafely ſay, that the little fetuſes are to be found in the ovaria, at leaſt a year before theſe animals ſeek each other for the purpoſe of generation: they do not, therefore, paſs from the male to the female during the act of fecundation.

Here it is proper to anſwer a queſtion that may be aſked. It appears from the obſervations of naturaliſts, that theſe ſeveral ſpecies of amphibious animals begin to propagate the ſecond year. And it is probable, that they continue to do this as long as they live; that is to ſay, for a conſiderable ſeries of years; we have at leaſt Roeſel's authority for believing that frogs live ten years or longer (*a*); and it ſeems likely, that toads come near them in this reſpect. Let it then be ſuppoſed that they propagate for nine years. The females will exclude nine ſucceſſions of fetuſes. If we examine the females during the firſt year of their growth, we ſhall not find any fetuſes in the ovaria. We are not able to diſtinguiſh any before the ſecond year, when two ſets appear, viz. the mature ones, thoſe which are to be brought forth that year, and the immature ones, which will be produced the ſucceeding year. That year the third ſucceſſion of fetuſes becomes viſible,

(*a*) If any concluſion may be deduced from the curious account of a toad in the Appendix to the Britiſh Zoology, the term here aſſigned is too ſhort. The individual there deſcribed is ſaid, if I remember right, to have lived at leaſt thirty-ſix years. T.

and

and the fourth year the fourth ſucceſſion; and in this manner one ſucceſſion only every year. Now it may be aſked, whether theſe orders of fetuſes, which ſucceſſively appear in the ovaria, pre-exiſted in them without being viſible; ſo that they are only evolved, and rendered conſpicuous by time, or rather whether they are formed in ſucceſſion, a new order being annually generated.

I ſhould reply without heſitation, that as it is by no means proved, notwithſtanding the efforts of the modern favourers of Epigeneſis, that any ſuch formation of organic bodies takes place, either in the animal or vegetable kingdom, and as all Nature abounds with ſuch evolutions, according to the accounts of the moſt judicious philoſophers of this age, it is natural to ſuppoſe, that theſe orders of fetuſes, which annually make their appearance in the ovaria, are not ſucceſſively generated, but co-exiſted with the female, and are only unfolded, and rendered viſible in progreſs of time, by the ſupplies of nutritive liquor that come from the female. This co-exiſtence of ſucceſſive orders of fetuſes, which become viſible in the ovaria, is analogous to that which takes place in the limbs. Tadpoles have at firſt no legs. Theſe parts appear only when they are about to aſſume the lineaments which characterize the ſpecies. Shall we therefore conclude, that they do not exiſt at firſt, but are generated when the tadpoles approach their metamorphoſis? Is it not infinitely more philoſophical to ſuppoſe, that the limbs co-exiſt with the tadpoles, and are inviſible, only becauſe they are too ſmall to ſtrike the ſenſes? And if it is reaſonable,

able to adopt this opinion concerning the limbs, ſhall we not alſo admit it with reſpect to the fetuſes of theſe animals?

CX. XII. If I paſſed over in ſilence the ſingular, and I think, new opinion of Mr. Gautier, concerning the generation of frogs, I ſhould deviate from the ſincerity which every philoſopher ought to practiſe. In a publication entituled, *Obſervations ſur l'hiſtorie naturelle, ſur la phyſique*, &c. after ſpeaking of ſome ſmall worms, which he found in a veſicle in the cavity of the abdomen of the male, he adds, that they are the real efficient cauſe of generation. I muſt quote his own expreſſions: La grenouille male montée & fortement attachée ſur la femelle attend les inſtans que les oeufs s'ecoulent de la femelle: il jette alors ſes embrions tels que je les ai apperçus, ils s'attachent aux oeufs, & s'en nourriſſent pendant quelques jours, juſqu' a ce qu' ils ſoient èn etat de ſe nourrir d'alimens plus groſſiers. Ces embrions conſervent la même figure qu'ils avoient dans la veſicule du pere, pendant l'eſpace d' environ un mois, temps au quel ils quittent cettent figure, comme font les vers a ſoyé dans le cocon. Ils developpent leurs pattes poſterieurs qu'ils ecartent enfin: ce ſont ces pattes qui unies dans l'embryon, forment la queue du têtard embryon de la grenouille.

As the book was publiſhed in 1752, I have had ſufficient time to examine the diſcovery. My firſt ſtep was to ſeek for the bladder containing the worms in the abdomen of the male. It was eaſily found, being no other than the urinary bladder, as may indeed be collected

collected from the deſcription of the author; in this I moreover found the worms; they are about the thickneſs of a thread, of a yellowiſh white colour, without rings, a line and a half in length; they are almoſt perpetually in motion, and have one extremity attached to the inſide of the bladder. So far I agree with Mr. Gautier; but I cannot, with him, conſider the worms as the fetuſes of the frog, for the following reaſons, which I think deciſive. They are likewiſe to be found in the bladder of the female, whereas they ought to exiſt only in the male, if they were what the French naturaliſt ſuppoſes them to be. Secondly, I have opened an immenſe number of frogs during the time of copulation, but have by no means found theſe worms in all of them. Thirdly, They never exceed twenty in number, whereas the fetuſes, from my obſervations and thoſe of Swammerdam, appear to amount to about a thouſand in every female. Fourthly, After fecundation the male ought not to contain any, but I have found that the number is not leſſened. Fifthly, As theſe worms adhere to the ſuppoſed eggs, and feed upon them for ſeveral days, I muſt have alſo ſeen them, eſpecially as they are viſible to the naked eye. I can, however, truly aſſert, that notwithſtanding all my attention in examining the eggs, both externally and internally, I never could perceive any veſtige of them. I ſhall deduce a ſixth argument from the artificial fecundation ſpoken of in the next diſſertation; which conſiſts in touching the fetuſes extracted from the females with the ſemen of the male, though that fluid ſometimes appeared

peared deſtitute of living inhabitants, even when examined by the microſcope. Theſe reaſons oblige me to reject the pretended diſcovery of Mr. Gautier. I will not pronounce it to be a mere fiction: I will rather ſuppoſe, that ſome fallacious appearance has miſled him, in conſequence of his inexperience in obſerving frogs, and his ignorance concerning their internal ſtructure, though it is exceedingly obvious. That this is the caſe every naturaliſt will perceive, from the paſſage in which Mr. Gautier affirms, that the hind legs of the tadpole, by their union, form the tail of this animal.

> Baia che avanza in ver quante novelle
> Quante mai diſſer favole o carote
> Stando al foco a filar le Vecchierelle.

Thoſe who ſhall have the curioſity to read his book, will learn, that the female frog has no uterus, that the tongue is fixed to the anterior margin of the palate, that the kidneys of the frogs are alſo the teſticles, &c. It is not, therefore, matter of ſurprize, that Roeſel ſhould treat him with ridicule, and conclude, non ſolum itaque aſſerere audeo parum in anatomia Ranarum profeciſſe Gautierium, ſed addere etiam non ambigo, ipſas ranas eundem vix habere cognitas. And as the Pariſian naturaliſt, in the relation of his diſcovery, modeſtly obſerves, that if Pythagoras had made one equal to his, he would have ſacrificed another hecatomb to the Gods. The German obſerver retorts; Ego vero crediderim, ſi fieri poſſet ut Gautierius Pythagoræ quæ invenerit, enarraret, hunc

hunc ipſi non ſilentium biennii vel quinquennii, quod diſcipulis ſuis imponere ſolebat, ſed perpetuum eſſe injuncturum.

CHAP. VII.

EXAMINATION OF SOME RECENT OBJECTIONS MADE AGAINST THE SYSTEM OF THE PRE-EXISTENCE OF THE GERM IN THE FEMALE.

CXII. DR. Pirri, a celebrated phyſician and philoſpher at Rome, is the author of theſe objections. To a ſenſible work on the theory of putrefaction, wherein he declares in favour of the Count de Buffon's ſyſtem of generation, he has prefixed ſome reflections concerning the reproduction of organic bodies, in which he attempts to overturn the principal arguments of thoſe who adopt the hypotheſis of pre-exiſtence of germs. In the firſt place he quotes and attacks two diſcoveries, one made by Haller on the chicken (CVIII), the other by me on frogs, and mentioned in my Proſpectus. Let us beſtow a little attention on both, and begin with that which belongs to me. "The learned Abbé Spallanzani, a name now ſo dear to his country, ſays he p. 7, has related another fact not leſs intereſting than thoſe already mentioned, nor in appearance leſs favourable to the doctrine of Palingeneſis, or the pre-exiſtence of germs in the eggs of the female, and by which the importance of the function of the male is very much leſſened. He watched the inſtant when the eggs are extracted and fecundated by the male. During this operation he killed the female, and by the aid of the microſcope, that inſtrument which has ſo often impoſed both on our ſenſes and underſtanding, he found, that both the impregnated and the unimpregnated eggs, which lay in the uterus, contained a tadpole bent in ſuch a manner, that the tail was contiguous to the head. The young animal was diſtinguiſhed by its black colour. In the impregnated eggs it was alive and in motion, but in the others it lay in a profound lethargy." It was proper to tranſcribe the expreſſions of Dr. Pirri, as they by no means agree with my relation of the diſcovery in the Proſpectus. The reader muſt allow me to quote my own words. After remarking, that the impregnated and unimpregnated eggs of the frog are perfectly alike, and that the latter are not productive, I add, p. 51, " but this is far from being the caſe with the former. They loſe their round ſhape, and elongate without ſenſibly increaſing in bulk, though they afterwards manifeſtly grow. The ſurface of the whitiſh hemiſphere turns darker, and upon the black hemiſphere appears a longitudinal furrow, terminated by two proceſſes, which become gradually longer, and take the direction of the long diameter of the egg. The furrow and proceſſes grow, and in time nearly burſt out from the ſide of the egg, which yet retains the ſhape of an oblong globule, with a prominence on one ſide. Meanwhile the whitiſh hemiſphere dilates, and the black one is incurvated, while the prominence increaſes in length, and it now appears, though this is ſtill more diſtinctly ſeen afterwards, to be the tail of the tadpole: the curvature on the black hemiſphere is diſcovered to be the back, and the dilatation on the oppoſite ſide the belly. The other extremity now takes on the figure of the head; in the anterior part the veſtige of the eyes, which are yet cloſed, becomes conſpicuous: the two proceſſes, by which the animal adheres to bodies, however ſmooth, come in ſight, as well as the rudiment of the aperture of the mouth; and laſtly the gills, through which the blood may be ſeen to circulate.

The animal does not yet ſhew any ſigns of life or motion, when it is pricked with a needle, or ſuddenly expoſed to the rays of the ſun, even when collected into a focus, though it is afterwards ſenſible of theſe impreſſions . . . . .

Such are the phænomena which gradually appear in fecundated eggs, whence it is evident, that theſe bodies are not, as it has been generally ſuppoſed, eggs, but the tadpoles themſelves concentrated and folded up.

It is therefore clearly proved, that the tadpoles exiſt before fecundation; but this intereſting truth may be more fully ſhewed thus: the fecundated eggs differ not in any reſpect from thoſe which have not been fecundated, but the former are the tadpoles concentrated and folded up, the latter muſt therefore be the ſame; wherefore the fetuſes of frogs exiſt before fecundation, and to be evolved,

evolved, want only the fecundating fluid of the male."

Upon comparing this paſſage with the quotation of Dr. Pirri, it is eaſy to perceive, that at the time of writing, he had not the Proſpectus before him. Firſt he advances what is not true, when he aſſerts, that I made uſe of the microſcope, "*that inſtrument which has ſo often deluded both our ſenſes and underſtanding*;" except in the paſſage where I ſay, that I examined the inſide of the egg with the microſcope, I never ſpeak of it, having had indeed no occaſion to make uſe of it; for the eggs are ſo large, as to be capable of being examined ſufficiently by the naked eye. If even I had been obliged to have recourſe to this inſtrument, I hope I ſhould not have incurred the danger which the learned phyſician mentions. The microſcope has, indeed, ſometimes been the ſource of error, either when it was ill conſtructed, or in the hands of unexperienced perſons. But it has, nevertheleſs, when theſe inconveniencies have not been preſent, enriched, and every day enriches natural hiſtory with the moſt important diſcoveries. Whoever, indeed, ſhall queſtion this, will riſque the imputation of being ſuppoſed deficient in common ſenſe.

Secondly Dr. Pirri ſays, that I found both the impregnated and unimpregnated eggs to contain tadpoles. Theſe words ſuggeſt an idea very different from that which I have expreſſed. I do not ſay, that I have found the tadpole to exiſt as well in the latter as the former egg, but that both are nothing but tadpoles. The expreſſion of Dr. Pirri ſuppoſes the

the exiſtence of eggs, whereas mine entirely excludes them.

Thirdly, The tadpole was, according to the objector, ſo bent, that its tail was contiguous to the head, and it was diſtinguiſhable by its dark colour.

In my Proſpectus I do not ſay that the colour is entirely black, but that one hemiſphere of the tadpole, while it retains its round ſhape is black, and the other of a dirty white. With reſpect to the poſition of the tail, the author is far leſs accurate; for I never dreamed of aſſerting, that the tail is bent towards the head, but that it appears like a prominence or appendicula, and increaſes in length as the tadpole grows. This is confined to the impregnated tadpoles; for the others have no tail, though Dr. Pirri would make the reader believe, that I attribute this part alſo to them.

Fourthly, I found this difference, that in the impregnated eggs the tadpole was alive and in motion, whereas in the others, it was motionleſs, and in a profound lethargy.

I ſhall finiſh theſe ſtrictures upon the extract with obſerving, that I never mentioned any profound lethargy in unimpregnated tadpoles; and ſo far was I from ſeeing any motion in the others at firſt, that I expreſsly mention the contrary. Let the reader again peruſe the quotation from my Proſpectus, and he will find that theſe ſtrictures are juſt.

CXII. I ſhall now examine the objections of Dr. Pirri. They may be reduced to two, of which the following is the firſt. My diſcovery of the pre-exiſtence of the tadpole, reſts upon an obſervation on which I fully

 confided,

confided, as it has the authority of Swammerdam; from whom we learn, that in frogs fecundation does not take place in the uterus, but without the body, ſince the eggs are touched with the ſemen of the male, after they are diſcharged. The Roman naturaliſt, relying upon the remarks of Roeſel, ſuggeſts various doubts with reſpect to this obſervation. "How, ſays he, can we be certain that the eggs of frogs are impregnated after they are laid, when Roeſel himſelf confeſſes the uncertainty of this, having ſeen an inſtantaneous contact, which may afford room for ſuppoſing, that the ſemen was thrown into the uterus? (p. 15.)"

When I compoſed my Proſpectus, I was not unacquainted with the doubts of Roeſel, but I was aware, that they did not deſtroy the validity of the contrary obſervation, ſince that is poſitive. Roeſel himſelf was ſenſible of this, and he does not, therefore, in ſpeaking of this momentaneous conjunction, queſtion the fact related by Swammerdam, a circumſtance which Dr. Pirri ought to have known. But further, he does not only not queſtion what Swammerdam tells, but ſtrongly confirms his obſervation in another paſſage of his work, where he ſpeaks of the generation of the green frogs. He ſays expreſſly, that the male bedews the eggs with his ſemen, after they have been diſcharged by the female. "Simulac autem femella ova ſua per anum emittit, maſculus eadem ſuo conſpergit ſemine; id quod ipſe domi meæ non ſolum vidi, ſed iteratis etiam viſcibus fieri non ſine admiratione obſervavi (*a*).

(*a*) Hiſt. Ranar. p. 56.

And

And to render this external fecundation still more evident, he represents the male mounted upon the female in their natural colours, the eggs as they are discharged, and the seminal fluid bedewing them, Fig. 2. Pl. 13. It might have been expected from the ingenuousness of my opponent, that he should not have suppressed this important observation, or rather that he should have spared his objections. If he entertained any doubts concerning the mode of generation in these animals, I hope they will be dispelled by the four first chapters of this dissertation.

CXIII. In the second objection, I am accused of a paralogism in the relation of this discovery. In speaking of the pre-existence of the tadpole, by the word tadpole I mean the embryo of the frog, or the frog in a very small state, disguised under the appearance of the tadpole. This offends Dr. Pirri, who considers the frog and tadpole as two distinct animals. He concludes, that " to make use of this fact as an unanswerable argument against the system of Epigenesis, is a fallacy that has escaped the penetration of the Abbé Spallanzani. It consists in confounding the appearance of the tadpole with that which belongs to the frog; and falsely supposing, that the tadpole and frog are one and the same animal."

I was indeed apprehensive, on account of my confined talents, that my book on animal reproductions would contain mistakes, but not that to which my learned adversary objects. My confidence was grounded upon Swammerdam, Vallisneri, Roesel, and many other excellent writers, who all agree in con-

 sidering

ſidering the tadpole and frog as the ſame animal. We know that many inſects paſs through the three different ſtates of worm, nymph, and winged animal: and thoſe who are at all acquainted with natural hiſtory know, that theſe different ſtates do not conſtitute three diſtinct animals, but that the ſame animal aſſumes theſe various appearances; ſo that the inſect equipped with wings, exiſted under the membranes of the worm and nymph, from which, when it is freed, it iſſues forth in a ſtate of complete evolution. Swammerdam obſerves, that what the nymph is to the winged inſect, the tadpole is to the frog. For he found the frog in miniature under the diſguiſe of the tadpole, and the winged inſect under the cover of the nymph. Both continue in this ſtate only till they are arrived at a proper period or ſtate of maturity, when they throw off their old habiliments, and aſſume their proper form. From theſe obſervations I had reaſon to conclude, that the tadpole and frog are identical; and this might have ſufficed as a reply to the objections of Dr. Pirri: my ſuppoſition was moreover founded on certain obſervations, his was gratuitous. My eſteem, however, for this phyſician, who is advantageouſly known by ſeveral other publications, and my deſire to aſcertain a fact of ſo great importance, induced me to undertake a more particular and exact enquiry into the identity of the tadpole and frog than that of Swammerdam. To prove this completely, it is neceſſary to ſhew, that the internal ſtructure and organization of the parts of the tadpole continues the ſame in the frog. If we find in in each the ſame ſyſtem of arteries, veins, nerves, and muſcles, if the heart, liver, lungs, and other viſcera be unaltered, if there be no difference in the organs of ſenſe and the diſpoſition of the bones, there can remain no doubt concerning the identity of the animals.

CXIV. Soon after Dr. Pirri's book came to my hands, in the ſpring of 1777, a ſeaſon very convenient for the purpoſe, I began this enquiry upon the green aquatic frog, of which I treat in the firſt chapter. To be as brief as poſſible, I will only mention the bare reſult, beginning as ſoon as the internal ſtructure of the tadpole can be examined, and ending at the time when it takes on the appearance of the frog. On the twenty-ſixth day the inteſtines are diſtinguiſhable, though the integuments of the abdomen, rolled up in a ſpiral, and in the region of the thorax, the pulſation of the heart is perceptible. Upon opening theſe cavities, we find the meſentery reſembling in thinneſs and tenderneſs a ſpider's web; along it ſmall red ſtreaks wind, which, when viewed with the microſcope, appear to be arteries and veins. The kidneys, lungs, and liver are then very conſpicuous; to the laſt viſcus is appended the gall-bladder, full of a tranſparent liquor without any bitterneſs. The heart is conical, and conſiſts of an auricle and ventricle; a little nearer the head lies the bulb of the aorta, which is divided into two branches that are implanted in the muſcles of the breaſt. The deſcending aorta is alſo viſible, and the vena cava, together with the beginning of their ramifications. The dorſal and lumber vertebræ, as well as the cranium, have nothing of the confiftence of bone, and the brain is gelatinous, as are alfo the nerves; thofe arifing from dorfal vertebræ are very diftinguifhable. The noftrils are open, and the iris is of a golden yellow colour. If we take the eye out of the orbit and open it, we fhall find the aqueous and vitreous humours, and the chryftalline lens, which has fome confiftence and is very tranfparent. The gills, which in more advanced tadpoles, appear on the outfide of the body, are as yet lying under the integuments of the thorax.

On the thirty-fifth day the vifcera are the fame, but larger and more firm. The liquor of the gall-bladder is ftill tranfparent, but fomewhat bitter. The arteries and veins are of a deeper red, and by confequence more confpicuous. The cranium and vertebræ begin to turn cartilaginous, and the brain, fpinal marrow, and nerves are not fo gelatinous.

On the forty-fixth day, thefe feveral parts are further unfolded and firmer. The bile is bitterer, and the rudiments of the hind legs begin to appear. Somewhat too of the fore legs may be difcerned, but they are as yet buried under the integuments of the thorax.

The fore legs do not appear till fifteen or twenty days afterwards; for the time varies in tadpoles that are brought forth at once. In other refpects the ftructure continues the fame.

Nor does it effentially vary during the fubfequent days, when the legs being unfolded, and the tail growing fhorter, and being at length

length obliterated, the tadpole puts on the form of the frog.

This happens about the eightieth day, when the old integuments come away. And now the animal becomes a true frog, differing from the adult only in ſize. It does not, however, differ from the tadpole, but has the ſame organs and viſcera, the ſame ſyſtem of arteries and veins and nerves, the ſame conformation of bones, with numberleſs other parts which I ſhall not deſcribe, leſt the reader ſhould find me tedious. Theſe new obſervations confirm the identity between the tadpole and frog; they demonſtrate that Dr. Pirri, in combating this ſuppoſition, has contended againſt truth. If any remaining affection for this harmleſs miſtake ſhould ſuggeſt to him, that the gills and tail of the tadpole are not to be found in the frog, and that the frog has four legs, while the tadpole has at firſt none, let him recollect, that the chicken, at its firſt appearance within the egg, has the ſhape of a worm with a large head and long tail; that the heart has afterwards the form of a half ring; that incubation continues ſome time before the legs and wings ſhoot, and that it loſes the umbilical cord when it breaks the egg (*a*). And yet, notwithſtanding all theſe appearances of metamorphoſis, no one, I believe, has ever imagined the pullet in the egg, and the hen to be different animals. And ſo much concerning Dr. Pirri's objections to what I have advanced.

(*a*) Haller. Form. du Poulet.

CXV. Let us proceed to that which he has brought againſt the diſcovery of Haller. Deſirous of knowing the ſentiments of this great phyſiologiſt, between whom and myſelf there had ſubſiſted a friendſhip of long continuance, I ſent him Dr. Pirri's book, but it came to his hands at a time when he was oppreſſed with thoſe maladies which were ſoon to deſtroy him. Inſtead, therefore, of writing his ſentiments at length, he replied in theſe laconic terms, Berne Nov. 5, 1777, " Je vous abandonne ce M. Pirri; il eſt en bonnes mains, vous ſçaurez aſſez defendre la bonne cauſe de la Nature. Il eſt toujours temeraire d'attaquer des experiences par des raiſonnemens." Thus he impoſed upon me the taſk of anſwering Dr. Pirri, and I would certainly have undertaken it, if upon more maturely weighing the objection, and comparing it with Haller's diſcovery, I had not perceived, that I might ſafely defer it at leaſt, if not entirely decline it, without offending the objector himſelf. I would, therefore, beg the ingenious naturaliſt to read the relation of the diſcovery over again with greater attention; for from his extract and reply, he appears to have overlooked more than one eſſential circumſtance. This will become evident, if we compare the expreſſions of the original with thoſe of the extract.

" The yolk of the egg, concludes Haller, from his obſervations on the chicken, is a continuation of the inteſtines of the chicken; the internal coat of the yolk is continued with the internal coat of the ſmall inteſtines and of the ſtomach, and pharynx, and with the ſkin and cuticle. The external coat is the external coat of the

the inteſtine; it is continued with the meſentery and peritonæum. The cover which incloſes the yolk, towards the end of incubation, is the ſkin itſelf of the fetus." He then reaſons thus, "if the yolk is continued with the ſkin and inteſtines of the fetus, it muſt have co-exiſted with the fetus, and is really a part of it. But the yolk exiſted in the female, independently of any commerce with the male; therefore the fetus itſelf alſo preexiſted."

The ſame great writer expreſſes himſelf with ſtill greater preciſion in his phyſiology. B. XXIX. Sect 2. Denique directa demonſtratio adeſt, quâ oſtendes, certe in avibus, pullum in matre fuiſſe. Pulli enim inteſtinum continuatur cum vitelli involucro & adeo inteſtini interior membrana cum epidermide animalis, exterior cum cute; denique cum involucro vitelli eadem eſt.

Dr. Pirri uſes the following terms, "Haller having ſhewn that the membrane of the yolk, which pre-exiſts in the impregnated egg, is transformed by incubation into the ſmall inteſtines of the chicken, concludes that the chicken pre-exiſts in the unimpregnated eggs."

Let the Roman phyſician determine concerning the fidelity of his own copy. To this he adds another fact taken alſo from Haller, and endeavours to confute him. But the author did not conſider the latter as of equal weight with the former, which, therefore, ought to have been faithfully quoted.

I am aware, that theſe ſtrictures cannot be pleaſing to Dr. Pirri, more eſpecially thoſe which ſhew his inaccuracy in relating the

diſcoveries

diſcoveries of others. I would, therefore, willingly have omitted them, as well on account of the real eſteem I entertain for his merit, as ſome degree of friendſhip I have contracted with him ſince the publication of his book; in conſequence of which, I have been ſtudiouſly mild in my expreſſions, and have oppoſed things and not words to him. But, when I was treating this ſubject, to omit them entirely was impoſſible. I will morever obſerve, that I do not conſider theſe inaccuracies as intentional, but as proceeding from want of reflection, or rather of leiſure; for he confeſſes, that he compoſed his conſiderations on the reproduction of organized bodies in a few days. I cannot, therefore, but urge him afreſh to examine theſe two facts, which prove, that the fetus belongs entirely to the female, at greater leiſure, and with deeper attention; I wiſh that to theſe he would add the further proofs I have adduced in this and the following diſſertation. That he may be a good judge of theſe facts, he muſt moreover allow me to ſay, that he ought to be in poſſeſſion of the difficult art of making obſervations and experiments with ſkill. He may then repeat thoſe which I have made, and will be enabled to give his opinion concerning them with greater freedom and ſafety.

CXVI. The author likewiſe diſapproves of Mr. Bonnet's doctrine concerning the involution of germs, though he does not directly attack it, but declares that he does not ſubſcribe to it; at which I do not wonder, becauſe the opinion of every one is free, and Dr. Pirri manifeſts too great a partiality for the

the Epigenesis of the illustrious Buffon. I was, however, not a little surprized, when I came to read in page 32, a passage of the Genevese philosopher; whence it would appear, that he is an opposer of the system of involution. My surprize was increased, when I learned that the evolution of bodies is, by the confession of Mr. Bonnet himself, deduced from equivocal facts, and from premises that by no means lead directly to any such conclusion. I was instigated, by the desire of knowing how my illustrious friend would reconcile these contradictions, to write to him at Geneva, making him at the same time acquainted with Dr. Pirri's sentiments concerning my discovery, and the organic molecules of Buffon. I did not long wait for an answer; which, as the author desired me to publish it, I will transcribe in this place.

Genthod near Geneva.

Nov. 29, 1777.

" CXVII. I was not acquainted with Dr. Pirri's book. The short account you give me of it surprizes me exceedingly. Is it possible that the eighteenth century could have produced a writer capable of asserting, that the tadpole and frog are two essentially different animals? Can this writer ever have read Swammerdam? And yet how can he treat of frogs without reading, or at least occasionally consulting him? Was not your illustrious countryman Vallisneri, whom he must doubtless have seen, sufficient to shew him

him the falſehood of his opinion? To me, this ſtrange aſſertion of Dr. Pirri appears altogether incomprehenſible. It is probable, that ſome ſecret intereſt has led him into this miſtake. An opinion ſo ſingular, does not deſerve to be very anxiouſly confuted. Every naturaliſt will excuſe you from taking much pains for that purpoſe.

"You inform me that Dr. Pirri declares, that my reflections on organized bodies have not perſuaded him of the pre-exiſtence of germs. I am not ſurprized that a naturaliſt, who believes the tadpole and frog to be two diſtinct animals, ſhould not be ſatisfied with my proofs. I ſhould be much aſtoniſhed if he were.

Your partizan of Epigeneſis will ſurprize thoſe who have not ſufficiently reflected on the influence of opinion. He owns on the one hand that you have fully proved the organic molecules of Buffon to be true animalcules, yet on the other he maintains that ſuch molecules exiſt, though they are inviſible. If they are not perceptible, how does he know that they exiſt; you tell me that he deduces this concluſion from conſequences. I ought to be made acquainted with theſe conſequences if I am to judge of their force. But the reaſoning of this author does not incline me to think favourably of his ſkill in logic.

A miſtake in reaſoning may indeed eaſily be forgiven, but want of accuracy and fidelity in quoting authors, is not readily to be excuſed. He attacks the involution of bodies, by mutilating a paſſage of my *Corps organiſés*; he might doubt of his cauſe from this alone, if he could form a proper judgment of his own

own conduct. I well know the arguments, ſays he, uſually adduced in proof of the poſſible tenuity of matter; nor am I unacquainted with the geometrical demonſtration of its infinite diviſibility. But I alſo know that theſe are mere illuſions, to ſurprize the imagination, and cloud the reaſon, as Mr. Bonnet has ingenuouſly confeſſed, Art. CXXVII. of his *Corps organiſés*, where he thus expreſſes himſelf concerning involution. The infinite diviſibility of matter by which involution, or the hypotheſis, which ſuppoſes one germ to be contained within another is ſupported, is a mathematical truth and phyſical falſehood. Every body is neceſſarily finite; all its parts are of neceſſity fixed and determined"—Who would not conclude from this extract, that I was combating the doctrine of involution? Yet this is the very paſſage in which I endeavour to prove its poſſibility.

Dr. Pirri, in order to perſuade his readers that I concur with him in opinion, dexterouſly ſeparates four lines from the paragraph, ſuppreſſes all the reſt, and then praiſes my ingenuouſneſs: I am ſorry I cannot return the compliment, but the truth is, that he makes me aſſert juſt the contrary of what I was endeavouring to prove: My words are, " the hypotheſis of involution is not without probability, but it is not neceſſary to ſuppoſe an endleſs involution, which would be an abſurdity; the infinite diviſibility of matter, which might furniſh ground for maintaining ſuch an opinion, is a mathematical truth, and a phyſical falſehood; every body is neceſſarily finite, all its parts are fixed and determined." I then proceed thus, " we are utterly

utterly ignorant of the fartheſt diviſion of which matter is capable; and this ought to prevent us from conſidering the involution of germs as impoſſible. We need only open our eyes and look round us, in order to learn that matter has been prodigiouſly divided. The ſcale of corporeal beings is the ſcale of this diviſion. How many times is mould contained in the cedar, the mite in the elephant, the aquatic flea in the whale, a grain of ſand in the terraqueous globe, and a globule of light in the ſun? An ounce of gold can be drawn by human art into a wire, eighty or one hundred leagues in length. The microſcope ſhews us animals, of which ſeveral thouſands do not equal the ſmalleſt grain of duſt. An hundred ſuch obſervations might be mentioned, and ſhall we, without heſitation, pronounce the theory of involution to be abſurd?

In the CCCXLIInd paragraph I treat profeſſedly on involution, and tranſcribe a long paſſage from Bourguet, in order to deſtroy the force of thoſe calculations by which the celebrated Hartſoeker affected to overwhelm the imagination. How then could Dr. Pirri fail to perceive, that ſo palpable a deviation from good faith would diſcredit his book?

I am yet more aſtoniſhed at another paſſage, in which this author has the aſſurance to aſſert, that "according to Mr. Bonnet's own confeſſion, involution of bodies is a ſyſtem founded on equivocal facts, and on obſervations that do not directly lead to any ſuch concluſion." An aſſertion ſo pointed, and at the ſame time ſo falſe, can impoſe on thoſe only who have never read me; for who among my my readers does not know that I have ever regarded the evolution of organized bodies, as eſtabliſhed upon the moſt unequivocal facts, and the moſt concluſive obſervations? All my works are full of the doctrine of the evolution of organized bodies; no author has ſaid more concerning it, or endeavoured to confirm it by ſtronger proofs. It ſeems morally impoſſible, that Dr. Pirri could have continued in his miſtake a ſingle moment in a matter ſo evident; and therefore ſince he puts into my mouth a propoſition, which he knows to be repugnant to my way of thinking on this ſubject, I may ſafely conclude, that his book was not dictated by the pure and diſpaſſionate love of truth. But I have beſtowed too much attention on an author ſo regardleſs of public eſteem, as to expoſe himſelf voluntarily to the heavy charges of ſuppreſſion and bad faith. I think you ſhould but juſt mention his work, for if you confute him at length, you will confer upon him a degree of celebrity, to which he is by no means intitled."

END of DISSERTATION I.

A DIS-

A

DISSERTATION

ON THE

ARTIFICIAL FECUNDATION OF CERTAIN ANIMALS.

CHAP. I.

ARTIFCIAL FECUNDATION OF THE TERRESTRIAL TOAD WITH RED EYES AND DORSAL TUBERCLES.

CXVIII. THE firſt attempt to effect artificial fecundation was made by my immortal countryman, Malpighi; having taken the eggs out of the ovaria of the butterfly produced from the ſilk-worm, he moiſtened them with the ſeminal liquor of the male. The event did not indeed correſpond to the wiſhes of the curious naturaliſt, for the eggs proved barren. The learned Bibiena, formerly profeſſor at Bologna, was in like manner diſappointed when he repeated and varied the experiments of his illuſtrious fellow-citizen (*a*). Such a project, however, though it fail in butterflies, does not ſeem unlikely to ſucceed in thoſe animals in which fecundation takes place without the body of the female, as in frogs and toads. Hence

(*a*) Act. Acad. Bon. T. 5. Part. I.

my

my celebrated friend Mr. Bonnet, ſince I communicated to him in 1767, my diſcovery of the pre-exiſtence of the germ in the frogs, has never ceaſed urging me to attempt the artificial fecundation of this animal. And as I conceived that if this experiment ſhould ſucceed, it would throw new light upon the natural hiſtory of animals, and more eſpecially upon generation, I reſolved to undertake it. When I publiſhed my Proſpectus in 1768, I gave an intimation of my deſign. Other occupations, however, prevented me from carrying it into execution till the ſpring of 1777, and the preſent year (1780) in the courſe of which I enjoyed more leiſure. The animals mentioned in the five firſt chapters of the preceding diſſertation, were the ſubjects of my experiments.

CXIX. I began with the terreſtrial toad with red eyes and dorſal tubercles, which begins earlieſt in the ſpring to propagate its ſpecies. I have already obſerved, that the female, with the male on her back, diſcharges ſlowly at the anus two ſhining viſcid cords, full of black globules. Theſe globules are minute tadpoles, which the male fecundates at the time of expulſion, by beſprinkling them with ſemen (XLV, XLIX, L, LI, LVII). As therefore the tadpoles are at this period beſt diſpoſed for fecundation, I tried to effect it in the following manner. Juſt before parturition, of which I was apprized by the exceſſive ſwelling of the belly, I parted the male from the female, and ſet the latter by herſelf, in a veſſel full of water. In a few hours the cords began to appear; as ſoon as about the length of a foot was excluded,

 I cut

I cut them off, and left one in the veſſel, while I took out the other, in order to wet it with ſemen, which I procured from the male that had been juſt ſeparated from the female. It is eaſy for any one who has the ſlighteſt ſkill in comparative anatomy, to find the ſeminal veſicles. In this animal, they lie below the teſticles, and cover part of the kidneys. At the time of coupling, they are always full. I laid open the veſicles, and receiving the liquor which had the tranſparency of water, into a watch-glaſs, I ſpread it on the piece of cord with a pencil; but the quantity was only ſufficient to go over two-thirds; after the operation, I placed this piece in a veſſel of the ſame water as that in which the unimpregnated portion lay. This experiment was made on the 16th of March; as the weather was rather cold, and conſequently unfavourable for the evolution of the tadpoles, I was forced to wait the longer in ſuſpenſe for the event, about which I was not a little anxious. I examined the cords with the cloſeſt attention ſeveral times a day, without perceiving the ſmalleſt difference for the firſt five days. In both the mucus was equally enlarged, as well as the tadpoles: and they retained their globular ſhape. It was not till the ſixth day that I began to conceive hopes, that the application of the ſeminal liquor had not been effectual. Many of the tadpoles comprehended in the two-thirds of the cord, over which the pencil had paſſed, began to aſſume an elongated figure, while the others preſerved their round form, as did alſo thoſe of the piece of cord left in the other veſſel. The ſeventh day was ſtill

more

more favourable to my hopes; for together with a manifeft elongation, an increafe in bulk became vifible; thefe appearances grew more evident every day, infomuch that there no longer remained any doubt of a confiderable evolution of the tadpoles. On the eleventh day, I perceived them moving within the amnion, and on the thirteenth they quitted the membranes, and fwam about the water. On the other hand, the unimpregnated tadpoles began to corrupt, and in time they were quite difcompofed, and turned putrid. Thus I called into life a number of animals, by imitating the means employed by nature. The reader will conceive the fatisfaction I received from the fuccefs of an experiment fo precarious and uncertain. He will eafily imagine, that I was not difpofed to ftop at my firft difcovery, but that I determined to repeat and vary my trials, in order to deduce fuch confequences as might illuftrate the fubject.

CXX. All the tadpoles in the portion of cord that was wetted with femen, were not evolved. They were in all a hundred and feventy fix, of which fixty-three fpoiled. This probably arofe from their not having been touched by the fecundating fluid. I propofed to repeat the experiment upon another piece of cord, which a female juft feparated from the male, was about to difcharge. As this piece was only five inches long, I could bathe it completely with the feed of two males. I may here remark, that if we wifh to find the veficles full, we muft open them at the time of copulation. The largeft quantity of feminal liquor afforded by a fingle male, is generally two grains, though it ſometimes amounts to almoſt three. When the males are not mounted upon the females, there is either no fluid at all in theſe reſervoirs, or only a very inconſiderable quantity: they are indeed ſo much ſhrunk, that there is ſome difficulty in finding them. But to return to my ſubject; I found that a greater quantity of ſemen cauſed more tadpoles to be evolved. The ſeaſon being now a little further advanced than at the time of my firſt experiment (CXVIII), the tadpoles began on the fifth day to take on an elongated ſhape, on the tenth they ſhewed manifeſt tokens of animation, and on the eleventh were ſwimming about the water. They were in all an hundred and ſeven, out of which number eight only failed; whether it was becauſe they had not been impregnated, or more probably becauſe they were vitiated. In the natural proceſs, among a great number of tadpoles, a few always ſpoil.

CXXI. In theſe two ſucceſsful experiments (CXIX, CXX), the cords had been diſcharged by the female into water. Natural fecundation always takes place in this element. But I had learned from obſervation, that it ſucceeds juſt as well when the animals are removed to a dry place. It was thus, that I was fortunate enough to diſcover the natural mode of impregnation (XLVII, XLVIII, XLIX). I therefore ſuppoſed, that artificial fecundation would ſucceed in the ſame circumſtances; a long portion of cord diſcharged in a dry veſſel was moiſtened with ſeminal fluid, and then put into water, along with another piece brought forth by the ſame female,

male, but not impregnated. In twelve days the tadpoles of the latter were half putrid, while thoſe of the former were evolved, and ſwimming about the water. While employed about theſe experiments, I was careful to obſerve whether artificial fecundation is ſlower in its effects than natural. Having two toads coupled, I waited till the female had diſcharged part of the cords, and the male had beſprinkled it with ſemen. I then removed the male, and cutting off the cords, cloſe to the anus, left them in the water. As ſoon as the female had diſcharged another portion of cord, (which happened in a quarter of an hour) I cut it off, and impregnated it with what remained of ſeed in the veſicles of the male; and this piece was put into the ſame veſſel with that which had been naturally fecundated, that it might be ſeen which would ſooneſt produce complete tadpoles. But evolution and animation kept an equal pace in both, an obſervation which I afterwards ſaw confirmed both in toads and frogs.

CXXII. My attempts to produce fecundation by artificial means, have hitherto been made, only when the tadpoles had arrived at the place deſtined by nature for impregnation and evolution. But ſuppoſe it was attempted within the body. We know that they are at firſt lodged in the ovaria, then paſs along the oviducts, and at laſt get into the uterus. Will artificial fecundation ſucceed alike in theſe three receptacles? Theſe reflections ſtimulated my curioſity, and I believe I have now data ſufficient for the ſolution of the problem. I began with the uterus. This organ is divided by a membranous partition into two cells, which are quite filled with tadpoles, as foon as they have paffed though the oviducts. They are inclofed in the glutinous cords, which are very much entangled; it is not, however, difficult to draw them entire out of the uterus with forceps, if they are managed gently. I opened the abdomen of feveral coupled females when the cords began to be excluded, for at this time the uterus is always full, and difentangling part of the mafs which lies in this vifcus, I bathed it with feed, and fet it in a veffel of water. The reft of the cords was put at the fame time into another veffel of water; but not one tadpole contained on the latter portion was evolved, whereas all thofe which had been impregnated became complete tadpoles. It is therefore to be inferred that thefe germs, by the time they get into the uterus, are arrived at fuch a ftate of maturity, as renders them capable of being fecundated.

While I was engaged in thefe experiments, I was eye-witnefs of an accident that deferves to be mentioned. Having often obferved the feminal liquor of the toad, I found it very full of fpermatic worms, which, like thofe of the frog, have an oblong fhape, and writhe their body as they move. Upon two occafions I have been greatly furprized at finding this fluid totally deftitute of inhabitants. I was induced to try, whether it is alfo deftitute of fecundating virtue, but I found that it was juft as effectual in this refpect, as that which moft abounds with thefe diminutive animals.

CXXIII. I next proceeded to try whether I could impregnate thofe germs, which in coupled females are often to be found in the oviducts.

oviducts. I succeeded upon many of the largest, those which lie nearest the uterus, but those which were situated in the narrow part, in the vicinity of the heart, where the other extremity of the oviducts is placed, all baffled my attempts. These different results may perhaps be thus explained. The gluten in which the tadpoles are imbedded, and which forms the two long cords, is produced as the germs pass through the oviducts, for they are totally destitute of it before they enter into these canals. Those tadpoles therefore which have passed along the greater part of the oviducts, and of course are nearest to the uterus, will be surrounded by most gluten. This substance has been appointed by nature for the first nourishment of the fetuses. If therefore they happen to be provided with a sufficient supply, as those are which lie nearest to the uterus, the aspersion of seed will not be ineffectual; but the contrary will happen with respect to those which have little or no gluten; for should they be animated by the act of fecundation, they will soon perish from want of nourishment. The necessity of gluten during the first period of evolution, is evident from another experiment, in which the tadpoles having been entirely stripped of it, were wetted with semen, but they came to nothing; nay, very few of those were evolved from which part only was taken. Upon opening one day a female during copulation, I found the tadpoles in the abdomen, instead of the uterus and oviducts. Their colour was black, as it is when they are arrived at a state of maturity, but they were without gluten, having never entered the oviducts. These fetuſes, after being moiſtened with ſeminal liquor were not evolved, as the reader will naturally ſuppoſe.

CXXIV. For the ſame reaſon thoſe taken from the ovarium would not grow. This viſcus is divided into two large lobes, each of which conſiſts of ſmaller lobes. All are full of little tadpoles, till the ſeaſon of amours arrives, when they ſeparate from their ſtalks, in order to go where nature has deſtined them. I beſprinkled many of thoſe tadpoles with ſeed when they were about to quit the ovarium, but in vain. This experiment ſuggeſted a curious idea. I had found that the abdomen of female toads, and this is alſo true of frogs, may be opened, not only without immediately deſtroying them, but frequently without preventing them from bringing forth their young, which are afterwards evolved, either in conſequence of natural or artificial fecundation. It is only neceſſary, that the female thus maltreated ſhould be kept in the dry; otherwiſe the water will enter in at the wound, and occaſion death before parturition takes place. Now what will happen, thought I, if the abdomen of a female ſhould be opened, and the ſeminal fluid be ſpread upon the ovarium? May it be expected that the tadpoles, after quitting this viſcus, paſſing through the oviducts and uterus, and being diſcharged at the anus will grow? Though I had little expectation from the experiment, and was determined to perform it. Having with this view perforated the abdomen of two females, I threw a drop of ſeed upon each ovarium from a ſyringe introduced through the wound. I moreover made

made a perforation in the membrane that invefts the ovarium, and injected another drop of feed, which muft have come into immediate contact with the tadpoles. One of the toads died five hours after the operation with the tadpoles in the ovarium, but the other difcharged part of them at the anus imbedded in gluten, as if fhe had been in a ftate of the moft perfect health. I kept them very carefully in water, but not one came to perfection; whence I was obliged to infer, that the gluten is fo indifpenfably neceffary for the nutrition of thefe delicate beings, that fhould it not be prefent at the time of afperfion with feed, they will perifh, even though it fhould be afterwards fupplied.

CXXV. Having eftablifhed upon a number of decifive trials, the poffibility of artificial fecundation with feed (CXIX, CXX, CXXI, CXXII, CXXXIII), I thought of trying whether the expreffed juice of the tefticles would anfwer the fame end. I fuppofed this not unlikely, the tefticles being the organs in which the feed is formed, or rather perfected and rendered fit for fecundation; but on account of their fmall fize in this animal, I could not hope to obtain it pure: and my only refource was therefore to fqueeze out the juice, and ufe this for my experiments. The tefticles in the prefent fpecies of toad are fituated at the upper part of the kidneys; they have a yellowifh colour and an oval fhape, but are a little gibbous on one fide, like thofe of the cock. At the feafon of generation they contain a brown, denfe, and fomewhat vifcid liquor. Having collected a little of this liquor in a watch-glafs, I moiftened with it about

about one hundred tadpoles, part taken out of the uterus, and part discharged by the female, after the male had been removed. I succeeded perfectly; the tadpoles grew, and I frequently repeated the experiment with equal success. Upon making a comparison between the efficacy of the seed, and the juice of the testicles, I found that both fluids produce their effects in the same time; but the latter did not cause the evolution of so great a number as the former, though I did not then know whether this arose from its inferior activity, or rather from its density, a quality that prevents its diffusion over an equal extent of surface.

CXXVI. In May 1780, while I was composing the present chapter, the fishermen brought me a species of toad, concerning the generation of which I have said nothing in the preceding dissertation, for I was totally unacquainted with it; I shall here give a short description of it, as it relates to the present subject. This species is smaller than the fetid terrestrial toad, but is of the same colour, if we except the belly, which is of a brighter white. But the diversity of manners and organization shews, that the two species are totally different. The skin upon the back of the fetid toad is rough like shagreen, the body is rather long, and the animal moves like frogs, by long leaps. The other has a smooth skin, a thick short body, and jumps a very little way. The male of the former species utters a sound resembling a man's whistle. The croaking of the latter is very low and indistinct, a clear proof that the organs of the voice are very differently constructed. The

structure

ſtructure of the parts of generation alſo differs; the female of the fetid toad brings forth two cords, but that of the other only one; the former derives its name from its bad ſmell, but the odour of the latter is at moſt not more offenſive than that of garlic. The male of the fetid toad throws his arms round the thorax of the female, but that of the new ſpecies claſps the abdomen. Hence it is apparent, that this toad ought not to be confounded either with that with which I have been contraſting it, nor with the *Bufo igneus* of Roeſel, in which the lower part of the body is ornamented with flame-coloured ſpots (CCCCLXIV); for in the ſpecies in queſtion, not a veſtige of ſuch ſpots is to be perceived.

The firſt that fell into my hands was coupled, and the female was diſcharging her cords. I had here an opportunity of admiring the curious ſpectacle deſcribed by Demours (CII). When they were placed upon my hand, they ſeemed at firſt a little timid, but ſoon afterwards the female continued her diſcharge, though I did obſerve that the male uſed his hind legs to extract the cord. I ſaw, however, what he did not obſerve, I mean, that the male ſprinkled the tadpoles from time to time with ſemen emitted from the anus, which in this ſpecies is terminated by a point. I watched this phænomenon for about thirteen minutes, and then reſolved to open both the male and female, for the ſake of the following experiments. Having extracted the remainder of the cord, which meaſured about a yard and a half, I divided it into four portions: One was left untouched, the ſecond

was

was moiſtened with the ſeed remaining in the veſicles of the male; the third with juice expreſſed from the teſticles; and the laſt with two drops of the ſeed with which the male was impregnating the cord as it was excluded, which I had intercepted in a watch-glaſs. Theſe four pieces were ſet in ſeparate veſſels of water, and in a fifth was placed that piece of cord which had been already fecundated. The reſult was what I imagined it would be, from former obſervations. Except the untouched piece of cord, all the reſt were animated with a number of tadpoles; whence we may draw two inferences, firſt, that in this freſh ſpecies fecundation is alſo external; and ſecondly, that it may be effected by art, either by means of the ſeminal fluid, or the juice of the teſticles. Four other couples were afterwards brought to me, two of which ſerved to confirm the foregoing obſervations, and the two others for experiments, which I ſhall relate in another place.

CXXVII. I ſhall finiſh the preſent chapter, with a ſhort digreſſion concerning the mode of propagation in this new ſpecies. The cord, both in the oviducts and uterus, is full of black globules, which whether examined internally or externally, exhibit characters perfectly reſembling thoſe of the tadpoles of other ſpecies, when they lie in the oviducts or uterus. If the impregnated globules be compared with the unimpregnated, they will likewiſe be found to bear an exact reſemblance both internal and external. But in time the latter become white, the ſurface grows wrinkled, and chopped in various places; the water diſſolves them, and they at laſt fall to pieces,

pieces, and are quite decompofed. The appearance of the fecundated globules changes alfo in about a day, but in a manner totally different. Upon the furface may be perceived two furrows, which meet to form an angle. Above the angle a prominent filament is feen, and at the fame time the globule increafes in bulk and length, while the end grows finer. During the following hours, the furrows become deeper, the filament projects further, and the point grows longer; on each fide of the furrows arife two fmall tumours, which before were not vifible. In about a day longer, the myftery concealed under thefe parts, which are gradually unfolded, is laid open. We find that the two furrows are the marks of the mouth, that the prominent filament is the fpine, that the pointed end is the tail, and the two tumours the gills. All thefe parts become more diftinct, when the young animals begin to move about the water. It appears therefore, from thefe obfervations, that the fame law which prefides over the birth and evolution of the fpecies mentioned in the preceding differtation, extends likewife to this, that the fetus belongs to the female, and that the afperfion of the feed of the male is a condition neceffary to the animation and evolution of the fetus.

CHAP.

CHAP. II.

ARTIFICIAL FECUNDATION OF THE WATER-NEWT, AND THE FETID TERRESTRIAL TOAD.

CXXVIII. As the fecundation of the water-newt does not take place without the body (CLXXX, CLXXXI, CLXXXIV), as in frogs and toads, I could not try to impregnate the fetuſes, after they have been diſcharged by the female, but was obliged to ſeek ſome other means of accompliſhing my intention. The female newt has no uterus; hence the fetuſes, after quitting the ovarium, paſs along the oviducts into the rectum, and are diſcharged at the vent. The ſeed of the male inſinuates itſelf into this aperture, and fecundates the fetuſes that lie neareſt to it. I have moreover ſhewn in the eighty-fourth paragraph, that the more remote fetuſes, thoſe which are ſituated higher in the oviducts, are not then impregnated; and that before this can happen to them, they muſt come down lower, and take the place of thoſe which have been impregnated and diſcharged. Thoſe therefore which occupy the oviducts, appeared to be the proper ſubjects of my experiments; but my attempts to fecundate them did not ſucceed. I proceeded in this manner; having laid open the oviducts of a female, and the *vaſa deferentia* of a male, I took out the ſeed, which in whiteneſs reſembles milk, and moiſtened the fetuſes

fetuſes lying high in the ducts; I then put them in water without delay. Like thoſe which are brought forth naturally, they ſunk to the bottom, where they were held by the tenacious gluten that inveſts them, but not one grew. The gluten gradually quitted them, and they burſt, and were diſſolved. I will not relate the many frequent repetitions and variations of this experiment, how I altered the doſe of ſeed, ſometimes letting fall a few drops upon the fetuſes, ſometimes bathing them ſparingly, and at others immerſing them in it, and as it were ſaturating them, but ſtill to no purpoſe. Their ſituation in the oviducts made me apprehend, that they periſhed for want of a ſufficient quantity of gluten, as is the caſe with tadpoles taken out of the upper part of the oviduct (CXXIII), and at the ſame time doubting on the other hand, leſt, if I took them from a lower ſituation, they ſhould be impregnated by the male, I had recourſe to an expedient which I thought would determine whether my apprehenſions were juſt. I was then (it was in April) in poſſeſſion of ſeven female ſalamanders, which I had kept all the winter in water, apart from any males; notwithſtanding this, they were beginning to diſcharge their fetuſes. I thought, therefore, of trying to fecundate theſe fetuſes, being certain that on the one hand they were inveſted with a ſufficient quantity of gluten, and that on the other they had not been impregnated. I have before remarked, that the males, during the ſeaſon of their amours, will emit their ſeed, if the belly be gently preſſed, and the females their young (LXXX, LXXXIV). By this

this easy method, I got these female newts to bring forth their young, and as they came away, took care to moisten them with seed. But notwithstanding this expedient, I never saw one of them grow, though I carefully kept them in water.

CXXIX. Mortified at so many disappointments, I was about to relinquish the attempt, under a persuasion that artificial fecundation would not succeed on newts, when a doubt suggested itself, whether I had not neglected a precaution essential to the success of these experiments. In another place I have remarked, that the male fecundates the female, not by injecting his seed immediately into her, but by throwing it into the water, whence it passes into the vent of the female, and impregnates all the fetuses that lie near the rectum (LXXX, LXXXI). It is therefore evident that the seed, when it produces its effect, is not pure, but diluted in water. Hence I was led to suspect that pure seed, which I had used in my experiments, was not fit for fecundation, (perhaps on account of its too great density) but that it must be diluted in water; I tried this method, and obtained results very different from those which have been just related. By gentle pressure upon the bellies of the seven females (CXXVII) that had been kept by themselves, I procured the discharge of twenty-seven fetuses. I bathed them all with water in which a small portion of seed had been diffused. Seventeen perished, the remaining ten did well, and during their evolution presented the appearances that are consequent upon natural fecundation (LXXXVII, LXXXVIII, LXXXIX).

By

By imitating Nature therefore in this circumſtance, I ſucceeded in fecundating this amphibious animal by art, as well on the preſent as on other occaſions; I loſt indeed many fetuſes, but this could not ſurprize me, ſince many periſh alſo after the natural impregnation (LXXXIX).

The juice expreſſed out of the teſticles diluted in water, anſwers the purpoſe of fecundation, though in this caſe too, about one-third only of the fetuſes is evolved. This failure, induced me to deſiſt from the proſecution of my experiments on this animal for the preſent. The fetid terreſtrial toad next engaged my attention, and indeed fully anſwered my expectation.

CXXX. We have ſeen, that in the terreſtrial toad with red eyes and dorſal tubercles, artificial fecundation ſucceeds not only on the tadpoles that have been diſcharged, but alſo in thoſe which lie in the uterus, and at the inferior extremity of the oviducts. We have moreover ſeen, that it may be effected either by the ſeed, or the juice of the teſticles. I can aſſure the reader, that the reſult of theſe experiments repeated on the fetid toad, was exactly the ſame.

But the great quantity of this ſpecies with which the fiſhermen ſupplied me, the abundance of tadpoles contained in the uterus, and the facility with which they may be fecundated, led me to attempt ſomething beſides the bare confirmation of my preceding obſervations. The diſcovery of the means of calling forth theſe animals into exiſtence, without the co-operation of the male, was an experiment pregnant with many others, which I was the more tempted to undertake, ſince the harveſt was yet not touched by the ſickle of philoſophy.

One of my principal views was, to aſcertain what would be the conſequence of trying to force Nature out of her ordinary path, either by adulterating the ſeed, and the juice of the teſticles, or by attempting to produce changes in the tadpoles. Such an inveſtigation promiſed to afford much inſtruction.

In the preceding experiments, I had uſed the ſeed, and the juice of the teſticles, immediately after it was taken from the male. The animal was fixed on his back, by means of nails driven through his hands and feet; after opening the abdomen, I ſought for the ſeminal veſicles, which, when the inteſtines are removed, appear behind the bladder, under the ſhape of two tumours; if the bladder be evacuated, a better view of the veſicles is obtained, at the ſame time the teſticles come into ſight; in this ſpecies, they are of a livid brown colour, and lie upon the kidneys. After having made this diſpoſition, I took, as my deſign led me, either the ſeed, or the juice of the teſticles, and bathed the tadpoles. I now thought of trying theſe fluids ſome time after the death of the animal. In another publication I have obſerved, that frogs, toads, and other animals very tenacious of life, may be inſtantaneouſly killed by inſinuating a ſharp inſtrument between two cervical vertebræ, and deſtroying a little of the ſpinal marrow. They fall inſtantly into convulſions, and ceaſe to live; an event that does not follow either decapitation, or any other treatment. Three hours after I had thus killed a

fetid

fetid toad, I opened the abdomen, and ſearched for the ſeminal veſicles, which were a little collapſed, not now containing ſo much ſeed as when the animal is alive and coupled. With this ſeed I bathed ſome tadpoles taken out of the uterus, whence all that were uſed in the experiments related in this chapter were taken. The event ſhewed, that the ſeed had not ſuffered by a continuance of three hours in the dead body. The ſame obſervation is applicable to another toad which had been killed five hours and a half; the ſeed indeed being in very ſmall quantity, could not be ſpread over many tadpoles. In the courſe of ſeven hours after death, the veſicles were ſo much ſhrivelled, that I could ſcarce get a drop of ſeed, which however retained its efficacy.

CXXXI. Two immediate conſequences may be deduced from theſe experiments; it appears in the firſt place, that after death the quantity of ſeed in the ſeminal veſicles gradually diminiſhes till it quite diſappears, agreeably to the laws of the animal œconomy; and in the ſecond, that this fluid, notwithſtanding its waſte, loſes nothing of its virtue, at leaſt in ſeven hours. But does it retain its prolific power any longer after the ceſſation of life? As dead toads would not, for the reaſon already aſſigned, enable me to ſolve this problem, there remained no other expedient than to take the ſeed out of the veſicles, and keep it in a phial or tube. Having procured all that theſe toads would furniſh, I put it in a little glaſs tube, and uſed it at different times. Six hours did not impair its efficacy, but at the ſeventh hour it ſeemed to be weaker, for only two-thirds of the tadpoles touched with it came to perfection. The eighth hour produced worse effects, and the ninth proved so hurtful, that out of sixty-five tadpoles, not one was evolved. The reader is not, however, to suppose, that this is always the exact measure of the time in which the seed becomes effete. By many experiments I have been taught, that this depends on the temperature of the atmosphere. In hot weather, seed taken out of its natural receptacles, becomes in six hours and a half unfit for fecundation. But in a cold season, it retained somewhat of its virtue eleven hours and upwards. The frequent alternations of heat and cold during last May, (1780) afforded me many opportunities of observing these different phænomena. Finding that cold favours the retention of the fecundating virtue, I thought of carrying the experiment further, by placing some seed in an ice-house. It now retained its efficacy twenty-five hours. When I reflect upon the nature of this liquor, it does not seem difficult to account for the effect of heat and cold. The seminal fluid, like other animal substances, is subject to putrefaction; now this is promoted by heat, and retarded by cold. In my second *Essay concerning the natural History of Animals and Vegetables*, I have spoken at large on the tendency of extravasated seed to become putrid, whence it loses the spermatic worms, its natural inhabitants, and acquires others of a very different kind, resembling the animalcules of infusions. The seed of toads runs sooner or later into the putrefactive fermentation, and on this account probably becomes [illegible] effete,

effete, fooner when the atmofphere is warm, than when it is cold.

I have faid before, that in hot weather the feed in fix hours and a half is rendered unfit for its natural ufe. I found that in this cafe, the thermometer ftands 18½ (*a*) deg. above the freezing point. This fuggefted to me the idea of trying what would happen to this fluid, if it fhould be kept for a fhort time in a higher temperature. From feveral trials it appeared, that the heat of 30 (*b*), and even 32 (*b*) deg. was not prejudicial, when the feed is fubjected to it for two minutes only; but at the 35th (*b*) deg. it was quite fpoiled.

CXXXII. But it is time to turn the reader's attention to the experiments on the juice of the tefticles, the fecond head of the firft enquiry propofed in the CXXXth paragraph; as before, I left the tefticles in the dead animal; but the event was very different. In the veficles the quantity of feed was fo much diminifhed, that in feven hours there fcarce remained enough for one experiment. The tefticles on the other hand of the fame individual preferved their juice fufficiently, and it was fit for fecundation. But what was expreffed from the tefticles of another male left dead for a day, was in fufficient quantity, but altogether effete. In the profecution of this enquiry, I found, that the continuance of the efficacy of this juice is connected with the temperature of the atmofphere, or, to fpeak more properly, with the time in which it began to grow putrid, as happens with refpect

(*a*) About 73 or 74 of Far. thermometer. (*b*) 30 = 99¼, 32 = 104, 35 = 110¾, nearly of the fame fcale.

to ſeed alſo (CXXXI). Hence in warm weather, the teſticles afford nothing fit for impregnation after nine or ten hours have elapſed; whereas when kept in an ice-houſe, their juice is efficacious after thirty-four hours. It appears, however, from a compariſon of this with the preceding paragraph, that in the ſame temperature this juice retains its virtue longer. Does this ariſe from its not becoming putrid ſo ſoon, at leaſt when it remains in the teſticles? I incline to this opinion, though I have not made any direct experiments with this view.

CXXXIII. The teſticles of the toad are ſo ſmall, that when expoſed to the air, they become ſhrivelled and dry, ſooner than they putrify. In the mean time they do not loſe their virtue, but the juice, as long as they retain any, is fit for fecundation.

As they dry, they grow hard, and aſſume the appearance of thin leather; when put into water, they reſume their former plumpneſs. When preſſed, they appear to be full of juice, but it will not cauſe the evolution of germs. And this is by no means matter of ſurprize, ſince the better and more active part of it, muſt evaporate during deſiccation.

The ſame effect is produced by too exceſſive heat, as when the teſticles are kept a few minutes in air or water warmed, to about the thirty-fifth degree.

If we combine the facts related in this and the foregoing paragraph, with thoſe of the CXXXth and CXXXIſt, it will appear, that the ſame cauſes are deſtructive of the qualities of the ſeed, and the juice of the teſticles. Indeed it cannot be otherwiſe, ſince the latter

ter fluid consists chiefly of the former; the same worms inhabit both. Hence it may be understood how it came to pass, that when I bathed tadpoles with the juice taken from the testicles of toads long kept by themselves, fecundation did not take place; this was not owing to a deficiency, but to the bad quality of the juice. It was to no purpose to try the juice of the testicles of young toads, before they are proper for generation. The failure arose from the want of seminal fluid, which, as is well known, is not produced either in men or animals before a certain age.

CXXXIV. In the artificial fecundation of both these species of toad, I had used seed or juice of the testicles not adulterated with any other substance. But having proposed in this chapter to enquire into the consequences that would follow the alteration of these liquors, it became me after the trials already instituted, to mix various matters with them. The slight difference between the seed and the juice of the testicles, rendered it probable, as in fact it happened, that their admixture would not prevent fecundation. Water too, though a fluid essentially different, did not seem likely to produce such an effect, since the seed is emitted into water, and necessarily mixed with it; and with respect to the newt, we have seen, that this is a requisite condition to impregnation (CXXIX). In the toad water heightened the prolific power of the seed; but I content myself at present with giving a simple intimation of this fact, and defer to the next chapter a particular account of it, as well as of the important deductions it affords. I here confine myſelf to other liquors. In opening the ſeminal veſicles, the ſeed which when unmixed has the purity and tranſparency of water, is always adulterated with blood that iſſues from the inciſion made in the abdomen: the mixture is then not unlike the waſhings of fleſh; I have uſed it for bathing a conſiderable quantity of tadpoles, and have found it juſt as good as pure ſeed. Further, blood, after being thus mixed, ſoon coagulates, and if taken away, leaves the ſeminal liquor as pure as ever. I have taken up ſome of the clots, and after rubbing the tadpoles with it, have found that it animates them, on account, no doubt, of the ſeed that adheres to it. The ſame remark may be applied to the other fluids of both male and female toads; the bile, the ſaliva, the juices expreſſed from the liver, lungs and kidneys, and in ſhort the urine itſelf, do not prevent the ſeed from producing its proper effect, notwithſtanding the laſt is thought by many to be poiſonous, or at leaſt corroſive and acrimonious. Human urine is not prejudicial, provided it is uſed in equal quantity. But the number of tadpoles became leſs when the portion of urine was increaſed, inſomuch that when it was uſed in double quantity, none were evolved. Vinegar agrees very nearly with urine. Human ſaliva is innoxious in whatever proportion it is employed. Here I may poſſibly be aſked, whether a real incorporation of theſe ſeveral liquors with the ſeed, took place: or rather, whether the ſeed formed a ſeparate and diſtinct body, as water poured into oil; in which caſe fecundation being effected, would not be a matter of ſurprize?

prize? I reply, with great confidence, that the mixture took place in the first of these ways, and never in the second. When I dropped these different liquors into the seed, I was careful to observe what changes happened, and that I might be better enabled to judge I used the microscope. The mixture seemed always to be attended with a real incorporation of the two fluids. When water was used, it appeared as limpid and homogeneous as if it had been water alone. When the seed was mixed with black vinegar, the mixture had a blackish colour, and when with white vinegar, it was somewhat less opake. In both cases the acid lost somewhat of its sourness, a certain proof that the seminal fluid, which of itself is insipid, had been diffused through the whole of the vinegar. The same thing took place when urine was tried.

The juice of the testicles after being mixed with the same fluids, lost nothing of its efficacy. And as it tinged these fluids with its own colour, it was reasonable to infer that it was incorporated with them as well as the seed. It appears therefore, that both these prolific liquors preserve their qualities some time after the death of the animal, both when pure, and when by being mixed among other matters they have sustained a considerable division of their integrant parts. But I shall speak more particularly concerning fecundation after a great, nay, almost infinite separation of the particles.

CXXXV. I now come to describe the various changes I have endeavoured to effect on tadpoles before fecundation, which was the other

other principal object of enquiry. Having killed several females in the way described in the CXXXth paragraph, I left the tadpoles for some time in the uterus. I wished to know how long after the death of the female, the young are capable of being impregnated; eight hours did not prove hurtful to them, nor twelve; but when bathed with seed, after thirteen or fourteen hours had elapsed, the difference became evident, for out of about two hundred, more than eighty perished. To the tadpoles of three toads, sixteen hours continuance in the uterus was fatal, but several taken out of that of a fourth came to perfection. This instance may serve to confirm a maxim adopted by the most able philosophers, according to whom a great multiplicity of trials is not merely expedient, but necessary, when we would establish any proposition. The induration of the tadpoles of the three first toads, the thickening of the mucus and its incipient putrefaction, were indications sufficient to make me apprehensive, lest the application of seed should not be attended with any effect. But I supposed, that in a lower degree of heat the tadpoles might be kept longer, for these experiments were made in June, when the thermometer in the shade stood at 20°—22° (*a*). It readily occurred to me, that recourse should be had to an ice-house, in which I had before kept the prolific liquors good for a considerable time (CXXXI, CXXXII).

My supposition was confirmed by experiment. Tadpoles left in the uterus of two

(*a*) 20=77, 22=81½.

females

females for forty-one hours in this degree of cold, and afterwards moiſtened with ſeed, came moſt of them to perfection. I cannot inform the reader what effect is produced by immerſing tadpoles in various liquors, not having had time for making ſuch experiments. The only one which I could try was water; and I ſhould not have imagined, that it would be ſo detrimental as it really proved to be, conſidering that the tadpoles are always depoſited and live in water. After tadpoles extracted from the uterus of a living toad, have been left four hours in water, the application both of the ſeminal fluid and the juice of the teſticles was quite ineffectual. They all turned putrid in a few days. The ſame event took place on other occaſions, after the tadpoles had lain for three, two, and even one hour in water. This unexpected phænomenon made me deſirous of aſcertaining the preciſe time, during which tadpoles may be kept in water without being rendered unfit for fecundation. I found it to be exceedingly ſhort, about thirteen minutes; many kept immerſed longer, never came to perfection, and after a continuance of fifteen minutes not one was evolved. This at leaſt was what I obſerved concerning ſevaral hundreds.

But how does water ſo ſoon damage theſe embryos? It is quite contrary to what happens when they are kept in the dry, whether they are left in the uterus, or placed in a veſſel; for it is certain, that a continuance of ſeveral hours will not under this circumſtance prevent the effects produced by fecundation. The beſt method of inveſtigating the cauſe

of

of the difference ſeemed to be, to obſerve carefully what happens to the tadpoles, and the mucus in which they are imbedded, while they continue in water. It has been already ſeveral times obſerved, that the mucus and tadpoles conſtitute two long cords, which the female ſlowly diſcharges. The cords at firſt fall to the bottom, but if the weather be warm they ſoon riſe to the top, and continue to float. This aſcenſion ariſes from an increaſe of bulk, which renders the ſtrings ſpecifically lighter than water. The dilatation is obvious to the ſenſes, and if the diameter be taken before they are put into water, and after they have been immerſed ſome time, it will be found to be enlarged. This change muſt, I think, be imputed to water inſinuating itſelf between the particles of the mucus. And my opinion is confirmed by the two following reaſons; the ſtrings, when left in the dry, do not increaſe in bulk; and if thoſe which have been immerſed be ſqueezed, water will ooze out of them. From theſe obſervations, I think we may account for the hindrance water occaſions to fecundation. It is obvious, that the ſeed muſt traverſe the mucus, in order to reach and impregnate the tradpoles. Now it muſt either inſinuate itſelf into the pores of that ſubſtance, or paſs along certain ducts provided on purpoſe by nature. When theſe openings, of whatever kind they may be, are free, as in the caſe when the cords are kept in the dry, the paſſage of the ſeed through the mucus will not be interrupted; but ſhould they be cloſed, its ingreſs will be prevented; this muſt be the conſequence of immerſion in water, and

and thus we come to underſtand why in a few minutes they are rendered incapable of being fecundated.

CXXXVII. The inſuperable obſtacle ariſing from the inſinuation of water, has been guarded againſt by Nature, when fecundation is left to the animals themſelves. The male toad, as well as the male frog, eject their ſeed upon the ſtrings, after they have been only a very ſhort time in water. And I have found, that the male always impregnates thoſe which have been juſt brought forth, and which conſequently have been juſt ſubjected to the action of water. Thus as if they could foreſee that fecundation would not take place after the tadpoles have lain ſome time in water, they never beſprinkle ſuch with ſeed. I have put ſome females in water by themſelves during the act of parturition, and after about a yard has been excluded, have admitted an equal number of males. The males aſſumed their proper ſtation without delay, and began to impregnate the tadpoles lying cloſe to the anus, without paying any regard to the others. I ſaid above, *as if they could foreſee*; for it is certain, that theſe animals have not ſagacity to diſtinguiſh between thoſe that are fit, and thoſe that are unfit for impregnation. The inſtinct allotted to them by Nature, prompts them to mount upon the female, and in this ſituation to impregnate the young. In ſuch a poſture, they neither do nor can impregnate any but thoſe ſituated near the parts of generation; and thus they unwittingly fulfil the great purpoſe of Nature, the propagation of the ſpecies. Hence it is evident, that although fecundation is external both in toads

toads and frogs, as it is commonly ſuppoſed to be in fiſhes, it is not effected when the fetuſes are ſeparated from the female, and lie at the bottom of the water. Of this truth, I have had the moſt ample proof in every ſpecies of frog and toad mentioned in theſe two diſſertations; for when many pieces of cord have been put in veſſels of water containing males, of which the ſeminal veſicles were quite turgid, not one fetus out of ſo large a number ever came to perfection. We muſt therefore conclude, that not a drop of ſeed was ever caſt upon them, ſince we have ſeen that a ſingle drop is ſufficient to impregnate great numbers.

CHAP. III.

ARTIFICIAL FECUNDATION OF THE TREE-FROG, AND THE GREEN AQUATIC FROG.

I WAS obliged to confine myſelf to a few experiments on the tree-frog, as well as on the water-newt, not on account of the difficulty of bringing the fetuſes to perfection, as was the caſe with reſpect to that diminutive quadruped, but on account of the few I poſſeſſed when I was engaged in theſe experiments. All the information therefore which I am able to give the reader, conſiſts in aſſuring him, that my attempts to produce artificial fecundation were ſucceſsful with four couples

couples of the tree-frog, both when the ſeed and the juice of the teſticles was employed.

I ſhall therefore proceed to the green aquatic frog, a ſpecies with which its prodigious abundance in the plains about Pavia has enabled me to make as many trials as I could wiſh. But in the firſt place, let me dwell a moment upon the deſcription of the parts of generation belonging to the male; thoſe of the female, have been already deſcribed in the firſt chapter of the foregoing diſſertation.

After the abdomen has been opened, and the inteſtines removed, the bladder, the ſeminal veſicles, and the teſticles appear in ſight. The firſt of theſe parts lies very near the anus, and is apparently divided into two lobes, though it has in reality only one cavity, for it may be evacuated by opening either lobe. When the bladder is full, it partly covers the ſeminal veſicles; but when the urine is evacuated, they are ſeen very diſtinctly. They lie a little higher than the bladder, and conſiſt of a thin membrane, through which the ſeed is ſeen; it has the tranſparency of water. Each of the veſicles is provided towards the top with a long appendix, which is inſerted in the epidydimis, and may be called the *vas deferens*. Four teſticles have been ſometimes obſerved in the newt, an obſervation which I have had occaſion to verify. But frogs and toads are never ſo richly provided. In the preſent ſpecies there are two teſticles, which have the colour of the yolk of an egg, and are externally granulated like thoſe of toads; they are very full of juice at the ſeaſon of amours, as are alſo the ſeminal veſicles (CXX).

CXXXIX.

CXXXIX. I uſed both the ſeed and the juice of the teſticles for artificial fecundation. My firſt trials conſiſted in impregnating tadpoles, recently taken out of the uterus of living females, with the recent prolific fluids of the male. I afterwards uſed theſe liquors, after they had been kept and were approaching towards putrefaction, upon tadpoles in like circumſtances. Laſtly, I tried the effects of mixture with different ſubſtances. Theſe heads comprehend the experiments related in the foregoing chapters. The event was the ſame, with the exception of a few ſlight variations. The prolific liquors of the green aquatic frog preſerved their qualities longer than thoſe of toads; but they do not ſo well reſiſt the action of ſudden heat. The ſeed of toads, expoſed to a temperature denoted by 32° for a few minutes, did not loſe its virtue (CXXXI), but that of this frog did in my experiments. The prolific liquors of the frog preſerve their efficacy when mixed with more human urine than thoſe of the toad will bear. The tadpoles of the frog ſooner loſe the capability of being fecundated in the uterus of the dead female than thoſe of the toad. Such are the principal differences.

CXL. I now proceed to an account of ſome new experiments, which I hope will be acceptable to the reader. I had on all occaſions either completely covered the mucus which ſurrounds the fetuſes with the prolific liquor, by means of a pencil, or immerſed them in a quantity contained in a watch-glaſs, ſo that every part was moiſtened. But is it to be ſuppoſed that they muſt be ſo exactly covered? The queſtion was of import-

ance,

ance, and experiment alone could decide it. The mucus is not drawn out into long cords as in toads (XLV, LXVIII), but is moulded into balls or globules, of which each contains a tadpole. Theſe balls, when taken out of the uterus, may be eaſily ſeparated. I bathed ſome all round, others only over one hemiſphere, and others over one-third of their ſurface; they were immediately put into water. The animation of all theſe tadpoles proved, that it is not neceſſary to cover more than a part of the mucous ſpheres with ſeed. The tadpoles of this ſpecies are round; one hemiſphere is white, the other black (XIV). Does bathing the mucus which is oppoſite to the black or the white hemiſphere produce fecundation moſt certainly? Or is this an indifferent circumſtance? Experiment ſhewed, that which ever part is moiſtened with ſeed, the tadpoles are evolved alike.

In my laſt trials I had not bathed a ſmaller portion than one-third of the ſurface. But I did not ſtop here. I reduced the ſpace more and more, till there remained not above the point of a pen, and even of a needle, which I dipped into ſeed, and then brought once into contact with a ball; and yet fecundation as readily followed, as when the globules were covered all over. I was next curious to know whether a quantity of ſeed ſo inconſiderable would fecundate the tadpoles lying contiguous to the ſphere that I touched. I placed twenty-four globules in twelve watch-glaſſes full of water; the two globules in each glaſs lay cloſe to each other, and the viſcidity of the gluten kept them in this ſituation. I then touched, with the point of a needle dipped in ſeed, one ball in each glaſs. But as twenty-two tadpoles came to perfection, it appeared that every drop of ſeed had impregnated two. I repeated this curious experiment with the ſame quantity of ſeed, and alſo with a ſmall addition. In the former caſe the number of tadpoles that grew to perfection was double of that of the balls that were touched: in the latter it always was; nay, when two globules were brought into contact with that which was touched, the tadpoles of both were very often evolved. The ſeed therefore of the great aquatic frog is ſo powerful, that the ſmalleſt drop is ſufficient to impregnate ſeveral tadpoles.

CXLI. Conſidering that in this ſurprizing experiment the ſmall quantity of ſeed muſt have paſſed through the mucus, before it could fecundate the tadpoles, I thought it worth while to try what would be the conſequence of doubling the thickneſs of this ſubſtance, and uſing the ſame portion of ſeed. Having put with this view ſome ſingle globules in watch-glaſſes, I laid hold of the mucus with a ſmall pair of forceps, and ſtretched it about an inch. After fixing it in this ſituation, I touched the extremity once with the point of a needle dipped in ſeed. The tadpoles ſometimes were ſpoiled, but they, not infrequently, came to perfection, an evident proof that the drop of ſeed had traverſed the whole thickneſs of the gluten, for as it was extended in an horizontal direction, it could not be ſuſpected that it had run down the outſide.

The next experiment bears a cloſe relation to this. About fifty globules were put into a glaſs-tube hermetically ſealed at the bottom. The tube was placed in a vertical poſition; and a ſtratum about an inch thick of gluten was placed at the top; the gluten was ſo fixed, that it roſe towards the circumference, and was depreſſed in the center, and thus it formed a kind of funnel. Upon this was dropped a little ſeed, and as ſoon as it was loſt (which ſoon happened), the gluten was removed, and the tubes were placed in water. When the quantity of ſeed was not exceedingly ſmall, almoſt all the tadpoles were animated, at other times ſeveral ſpoiled.

I repeated the experiment, ſubſtituting the white of an egg in the place of the mucus. But then not one tadpole was evolved. When I put a little upon the globules, it grew putrid and communicated the ſame fermentation both to gluten and tadpole.

The two firſt experiments related in this paragraph, ſhew the wonderful efficacy of the ſeminal liquor of the frog, though in ſuch ſmall quantity in penetrating into the mucus, and paſſing through it without loſing its virtue. This phænomenon muſt be either owing to the pores of this ſubſtance, or to capillary tubes wrought by nature, in order to imbibe the ſeed and tranſmit it to the body of the tadpole. The white of the egg being deſigned for the firſt nouriſhment of the chicken, an animal widely different from the tadpole, muſt neceſſarily be conſtructed differently from the mucus, which affords nouriſhment to the tadpole. Hence it is eaſy to comprehend why the paſſage of the prolific liquor through the white is impoſſible, while it is ſo eaſy through the gluten.

 CXLII.

CXLII. But if we are ſtruck with ſurprize by the ſmall doſe of ſeed which is capable of effecting impregnation, the following experiments will very much increaſe this emotion. In the CXXXIVth paragraph I intimated that this fluid, when mixed with water preſerves its efficacy, but I poſtponed the particular diſcuſſion of this matter: I ſhall now enter upon it. In my firſt trial, equal quantities of the ſeminal liquor and water were uſed. Finding that this mixture was fit for my purpoſe, I doubled the proportion of water. In every experiment I employed what was afforded by the veſicles of one frog, which never exceeded three grains. But the effect was heightened very much by this increaſe of water, inſomuch, that twice the number of tadpoles was evolved: Not that the water concurred in producing this difference by any virtue of its own, but becauſe the ſeed was ſo much attenuated, that it might be ſpread over twice the number of tadpoles. For the ſame reaſon, when the water was in the proportion of four to one, three hundred came to perfection, whereas when theſe fluids were in equal quantities, not more than one hundred were evolved. This fortunate reſult gave me courage to try the mixture of a pound of water with three grains of ſeed. And here my ſucceſs was greater than I could have conceived. To my aſtoniſhment almoſt all the tadpoles furniſhed by the uterus of two females, were brought to life by being immerſed in this mixture.

Convinced that admiration ſometimes takes ſuch ſtrong hold of an object as to hinder reaſon from conſidering it properly, I

ſought

ſought to leſſen it by a reflection. When I put the tadpoles into water, they ſunk, as uſual to the bottom, where they were held by the mucus. Now did it happen that the ſperm, heavier perhaps than water, fell to the bottom, exactly upon the ſpot on which the tadpoles lay? If ſo, fecundation was not produced by ſeed diffuſed through ſo much water, but confined to a much narrower compaſs. By this ſuppoſition, ſhould it prove true, the wonder would be very much leſſened. The way I took to determine this queſtion was, to mix the ſeed of another frog with a freſh pint of water. Now ſuppoſing the ſpecific gravity of the former liquid to be greater, it would go to the bottom in the ſpace of an hour. At the expiration of this time I fixed tadpoles at various heights in the water. If we ſuppoſe that the ſeed fell to the bottom, it is obvious that thoſe only which lay there would be impregnated. This indeed happened, but at the ſame time all thoſe which occupied different ſtations were likewiſe impregnated, not excepting thoſe which were at the ſurface. Nor could I perceive that the loweſt had the advantage of being evolved in greater proportion. The conſequence is obvious, the ſeed did not fall to the bottom, but was diffuſed equally through the maſs. The firſt ſurprize therefore returns. Three grains of ſeed may be diluted in a pint of water, without loſing their ſtimulating power!

CXLIII. The reader will eaſily gueſs, that after having carried the experiment ſo far, I would urge it onwards, and employ quantities of water ſucceſſively larger, till the fecundating virtue ſhould be diminiſhed or deſtroyed,

 for

for it is natural to imagine that this muſt happen at laſt. I therefore next employed eighteen ounces inſtead of twelve, and the tadpoles placed at different heights did as well, and were nearly as numerous as before. But the product was leſs in two pounds of water, and about one-third leſs in three, four pounds were ſtill more prejudicial. But notwithſtanding this diminution of effect, I could not help wondering when I ſaw that even upon the addition of twenty-two pounds of water, a few tadpoles received life.

CXLIV. We have already ſeen how ſmall a quantity of pure ſperm ſuffices, a drop taken up by the point of a needle! (CXL). This truth appears with ſtronger evidence, when three grains are diffuſed in twelve, and in eighteen ounces of water (CXLII, CXLIII). But the facts which I am now to adduce, prove yet more forcibly that a quantity infinitely ſmaller will produce the ſame effect. How diminutive muſt be the particles of ſeed comprehended in a drop of water ſcarce viſible, and taken from a maſs of water of eighteen ounces, in which three grains of the ſeminal fluid have been diluted, the reader may imagine. They are, notwithſtanding, capable of producing fecundation. I dipped the point of a needle into this mixture, and touched ſeveral globules with it. The diameter of the drop taken up was about one-fiftieth of a line. To my great ſurprize the tadpoles frequently grew to perfection. And what added ſtrength to this emotion was, to find that thoſe imbedded in globules touched at one point, were evolved juſt as well,

well, and as ſpeedily, as thoſe of others immerſed in pure ſeed.

CXLV. Having ſeveral pounds of water, containing each three grains of ſeed, I was unwilling to lay them aſide before I had made ſome further experiments. I firſt tried, whether the water is rendered ſterile by the fecundation of a number of tadpoles. As many globules as a pound of water would contain were immerſed in it; they amounted to ſeveral thouſands. After they were taken out, others were thrown in; and theſe came to life juſt as well as the former; and ſo did others afterwards immerſed. I was weary of repeating the experiment before the water loſt its efficacy. I kept an account of the tadpoles of fifty frogs, which were ſucceſſively put into this water without impairing its prolific power.

I next tried, whether the fetuſes of frogs are brought to life ſooner by a long continuance in the water. A pound was divided into equal portions, and in one ſome globules were kept immerſed for a ſecond, and then put into pure water; at the ſame time, other globules were put into the other portion and left in it; but there did not appear to be any difference in the evolution of the tadpoles.

Laſtly, I wiſhed to know how long water, containing ſo ſmall a quantity of ſperm, preſerves its qualities. It preſerves them longer than pure ſeed. It would impregnate tadpoles thirty-five hours after the mixture, when the thermometer ſtood between 17° and 19° (*a*) in my apartment. In an ice-houſe, in which

(*a*) $17 = 70\frac{1}{4}$, $19 = 74\frac{3}{4}$ of F.

the thermometer ſtood (*a*) $3^\circ \frac{1}{2}$ above the freezing point. I have before obſerved, that putrefaction probably deſtroys the prolific power of the ſeed (CXXXI). And ſince it does not ſo ſoon run into this degree of fermentation, when divided by a large quantity of water, we eaſily underſtand how it comes to retain its virtue longer.

CXLVI. From the CXLth to the CXLVth paragraph, I have related experiments made with the ſperm of frogs, without mentioning the juice of the teſticles. But it muſt not be ſuppoſed that I have neglected to make uſe of it; the ſingularity and beauty of the enquiry in which I was engaged, required that it ſhould not be overlooked. I employed this juice in every trial mentioned in the preceding paragraphs; whenever I opened the ſeminal veſicles of a frog, I at the ſame time took out its teſticles. I was thus eaſily enabled to remark the reſults afforded by each of theſe fluids, and if there was any remarkable difference, it could not fail to ſtrike me; but no ſuch difference was ever perceptible. I may here clear up a doubt ſuggeſted in the CXXVth paragraph. I there obſerved, that the juice of the teſticles of the terreſtrial toad with red eyes and dorſal tubercles, impregnated in like circumſtances a ſmaller number of tadpoles than the ſeed. I was ſpeaking of it when mixed with other ſubſtances; and I ſubjoined, that it was not in my power to determine, whether the defect was to be aſcribed to inferiority of efficacy, or to its thickneſs, a quality which prevents

(*a*) $3^\circ \frac{1}{2} = 39$ or 40.

it

it from being ſpread over ſo large a ſurface. The addition of other ſubſtances, but eſpecially of water, ſhews, that the latter is the true cauſe. I have found on many trials, that the juice of the teſticles, as well of the frog as the toad, imparts to water as great efficacy as an equal quantity of the ſeed.

CXLVII. I muſt not omit to mention another circumſtance of ſome importance. The great abundance of each ſpecies of toad mentioned in the two preceding chapters, which has fallen into my hands in the courſe of the preſent year, has enabled me to make as many experiments with them as with the green aquatic frog; and I have obſerved no eſſential difference in the reſults. Frogs and toads conſtitute but one genus; they copulate and propagate the ſpecies in the ſame manner; the reſemblance of the organs of generation in both ſexes is very cloſe; the fetuſes are impregnated in the ſame way; they are alike imbedded in mucus: ſo many marks of affinity eaſily enable us to comprehend, why experiments ſo numerous and various afford correſponding reſults.

Before I conclude my account of theſe attempts to effect artificial fecundation, I muſt make two obſervations. Every experiment is not crowned with ſucceſs alike. Suppoſe, for inſtance, five hundred tadpoles to be bathed with the ſeed of a frog or toad, and other five hundred with that of another. Let the quantity of this fluid be the ſame in both caſes, and imagine the fetuſes to be taken out of the uterus. The effect will certainly follow; we may be ſure that the tadpoles will be evolved; but we cannot be equally ſure

that

that the ſame number, or nearly the ſame number, will be evolved; for it ſometimes happens, that the whole of one five hundred come to perfection, while half of the other or more fail. So it falls out likewiſe when they are left to nature, as I have frequently witneſſed when I kept theſe animals in veſſels, in order to diſcover the ſecret of impregnation, and any follower of ſuch purſuits may obſerve the ſame thing in the open fields. In May we may ſee in the ſtagnant waters, in which frogs celebrate their amours, ſome tadpoles that have lately quitted their mucus and are ſwimming about, while others are ſtill involved in it; but they ſhew ſigns of animation, and are ſeemingly eager to break looſe from their confinement. If we fix our attention upon the latter, we ſhall perceive in the heap globules not clear and tranſparent like thoſe which contain living tadpoles, but turbid, and of a muddy white colour; they have no motion, nor are they elongated like the others, but remain at reſt and preſerve their original round ſhape. The fiſſures upon the ſurface are evident ſigns, that the fetuſes will never grow to perfection. The ſame obſervation may be made upon the long ſtrings of toads. This partial failure may be traced to two ſources: it may ariſe either from the ſeed or the tadpoles. If every part of this liquor ſhould not be equally prolific, the whole will not produce fecundation. Part alſo of the tadpoles may be vitiated, and incapable of being impregnated.

My ſecond remark relates to the difference of time requiſite for evolution. Though they ſhould all be taken from the ſame ſituation, and

and all equally covered with ſeed, yet they will not all be equally forward in their growth. There will be ſometimes the difference of a few hours, and ſometimes of a whole day. It may be obſerved, that of the lateſt more fail than of the others. This circumſtance guided me in my enquiry, whether a ſmaller quantity of ſeed did not produce ſuch ſpeedy effects. This did indeed now and then take place, but my often repeated trials convinced me that it was but ſeldom. Upon the whole, thoſe which were impregnated with a larger quantity of ſeed were not more advanced, and in a few inſtances they were not ſo forward. Upon weighing the ſeveral facts which I obſerved, I was obliged to conclude, that a larger or ſmaller quantity of ſeed is an indifferent circumſtance in this reſpect. The tardineſs of evolution ſeemed to be owing to the cauſes ſpecified above.

CXLVIII. I will conclude the chapter, with an anſwer to a queſtion often aſked concerning artificial fecundation. Many lovers of experimental inveſtigations have enquired, whether the tadpoles and newts which I brought into life, exactly reſembled, both externally and internally, thoſe which are the product of pure nature; and whether I was certain, that they loſe the ſhape of a worm in order to acquire the form of the ſpecies. When firſt I was thus interrogated, I was acquainted only with the few reſults of my firſt experiments made in 1777, and conſequently could not give a ſatisfactory anſwer. The lineaments appeared to be very much alike, both in the products of art and thoſe of nature; but I had not then conceived any idea of of making a particular comparifon between them. After I had obtained my purpofe of bringing thefe animals to life, and they had grown to maturity, I was willing to enquire further, though I faw no reafon for fuppofing that artificial fecundation fhould alter the laws of nature. The comparifon was however made in the prefent year (1780), and I can confidently affirm, that upon the moft minute examination I have never difcovered the fmalleft difference, either in the external or internal parts; both the one and the other have undergone the accuftomed change at the time appointed by nature, the tadpoles becoming frogs and toads, and the young newts lofing their gills and producing their legs. It is therefore evident, that artificial fecundation does not produce the fmalleft alteration in the œconomy of thefe animals, but that every thing proceeds in the fame regular tenour as in natural fecundation.

CHAP IV.

REFLECTIONS.

HITHERTO I have feldom departed from the duty of the mere hiftorian; a duty which I have difcharged with the greater pleafure, fince the facts I had to relate had the recommendation of novelty. But it is now fit that I fhould affume the character of the philofopher; that I fhould analyze and compare thefe facts, and employ them not only for the further illuftration of fome truths which I have eftablifhed relative to generation,

tion, but alſo for the explanation of different phænomena accompanying this admirable function. The following reflections will, I hope, contribute to this purpoſe.

I. That the females of ſeveral amphibious animals contain in their uterus fetuſes compleatly generated before the approaches of the male, is one of the principal propoſitions ſhewn in the preceding diſſertation. It is proved at length in paragraphs XVIII, XIX. XXX. LIV, LV, LVI, LVII. LXXII. XCI. It is confirmed in the CXXVIIth paragraph of this diſſertation. Hence by paragraphs CX, CXI. are deſtroyed the two famous ſyſtems of the *Epigeneſiſts* and the *Vermiculiſts*. The facts related in the preſent diſſertation furniſh a new proof, or rather a demonſtration of their falſhood. Let us take a ſhort view of the queſtion. Thoſe who conſider the ſpermatic worms as the immediate authors of generation, muſt of neceſſity ſuppoſe, that they exiſt in the ſeed whenever fecundation takes place. It is therefore obvious, that according to this ſyſtem the ſeminal fluid will be unprolific, when it is totally deſtitute of theſe inhabitants. And indeed its advocates admit this. But it does not agree with facts. Though in the ſeed of the animals ſo often mentioned vermiculæ may be generally obſerved, yet in that of two toads they were entirely wanting; but it impregnated tadpoles, juſt as well as the ſeminal fluid of other individuals, however well peopled that might be (CXXII). In the next place, when the ſeed of the frog or the toad is mixed in equal quantity with human urine or vinegar, the worms are all deſtroyed.

The

The ſeed, however, as I obſerved in the proper place (CXXXIV), did not loſe its prolific power. It retained this power when a few grains were mixed with twelve ounces of water, and even with eighteen, though I could not diſtinguiſh a ſingle ſpermatic worm, ſo thinly were they diſperſed through that large body of water. It ought likewiſe to be obſerved, that in ſeed long kept, which yet anſwered the end juſt as well, I often found the worms dead, and floating upon the fluid of the veſicles, and likewiſe upon the juice of the teſticles. Laſtly, I could eaſily bathe tadpoles with ſeminal liquor abounding in worms, and yet prevent them from having the leaſt concern in fecundation. When a drop is put upon the object-glaſs of the microſcope, every part of it is full of worms. But when a little evaporation has taken place, they begin to quit the circumference, and aſſemble towards the center, as the animalcules of infuſions uſually do. As evaporation advances, they continue to fly towards the center, though many are caught, and die in the dried part of the drop. At this time I could take up with the point of a needle, at the edge of the liquor, many drops that were entirely without worms. That I might be quite certain, I placed what I took upon another object-glaſs; and when I found that it was quite free from worms, I touched ſeveral tadpoles, and found that they came to perfection. My long experience in the world of microſcopical animals, whether belonging to man or animals, will, I hope, vouch for me, that I was not deceived in this delicate inveſtigation.

investigation. These facts prove then irrefragably, that the system of Lewenhoeck and his followers is false.

CL. This system, it is true, had been attacked by many naturalists, among whom Vallisneri, my illustrious fellow-citizen, is entitled to particular praise. But if we consider their arguments we shall find that they are rather alluring, if I may use the expression of the great Verulam, than convincing. Haller alone, the immortal Haller, has, in my opinion, produced an insuperable objection against the spermatic worms, considered as the authors of generation. It is deduced from his celebrated discovery of the continuation of the membrane of the yolk with the intestines of the chicken. Now as the yolk existed in the hen before she received the cock, so did the chicken likewise before fecundation. Whence he drew this corollary, that the chicken had no dependence upon the spermatic worms, which pass from the cock to the hen. This inference, however just and direct it may appear, has not escaped the subtle cavils of some, who have objected the possibility of an inosculation between the membrane of the yolk, and the spermatic worm coming from the male, whence they derive the continuation between that membrane and the intestines, observed by the physiologist of Berne. He, however, in his great work, and, after him, the acute Bonnet, have shewn how unphilosophical such a supposition is. But it is certain, that no such notion can be applied to my observations on tadpoles, both because they exist before fecundation, and because in

experi-

experiments, in which artificial fecundation was produced (CXLIX), the ſpermatic worms had no concern.

CLI. This ſingular mode of impregnation, equally demonſtrates the falſhood of Epigeneſis, or of that ſyſtem, which has been raiſed from the dead, protected and careſſed by Buffon; who, by means of his *organic molecules*, has created an imaginary organic world, as his countryman Deſcartes had before conſtructed the whole maſs of exiſtences, both organic and inorganic, with his ſubtle matter. The ſpermatic worms having firſt ſtruck his ſenſes, and being thence transferred to his fervid and creative fancy, loſt their former name of animals, and acquired the new title of organic molecules. What violence has been offered to nature by this unintelligible metamorphoſis, I need not now attempt to ſhew, having treated the ſubject at length in my ſecond *Eſſay on the Natural Hiſtory of Animals and Vegetables*. But if I admit, for a moment, the reality of this extravagant opinion, it will follow, that whenever the ſeminal liquor of animals is deſtitute of worms, it muſt, according to this renowned naturaliſt, be alſo deſtitute of organic molecules. And as fecundation and generation depend upon the various combinations of theſe molecules, ſeed that does not contain any of them muſt be unfit for ſuch purpoſes. In the inſtances mentioned in the CXLIXth paragraph, the ſeed of frogs and toads muſt therefore have been unprolific, which is directly repugnant to fact.

CLII.

CLII. II. The confutation of theſe ſyſtems confirms more and more this important truth, that the young belong originally to the female, while the male only furniſhes a fluid, which determines them to aſſume motion and life. I would not indeed aſſert, that theſe little organized bodies are without motion before they experience the action of the maſculine liquor. From the time at which the tadpoles in the ovaria fall under the cognizance of the ſenſes, till they are about to be impregnated, they grow conſiderably, inſomuch that they are many times larger at the latter than at the former period. Now growth implies nutrition, nutrition the circulation of the fluids, and circulation depends on the pulſation of the heart. I therefore conceive, that, previouſly to the influence of the ſeed, there was a beginning of motion and life, but in a degree exceedingly dull and languid, from the extreme ſlowneſs of the movement of the fluids; but this idea occurred to Haller and Bonnet before me. Hence tadpoles would never be ſo rapidly evolved, or atttain that ſenſible animation, which we denominate life, if they were not ſubjected to the influence of the ſeminal fluid. This raiſes them from a ſtate of apparent ſhapeleſſneſs and immobility, and produces a due unfolding of the limbs, and evident motion, and active life.

But the ſeed, in order to exert its power upon tadpoles, muſt penetrate into them; for it is not likely that it can produce animation by mere impreſſion upon the ſkin. Are the paths by which it enters perceptible to the eye? I have beſtowed much attention upon this queſtion, and the repeated inſtances of Mr. Bonnet have ſtill further engaged me in ſo difficult an enquiry. I therefore examined ſome mucilaginous globules diſcharged by frogs, with a weak microſcope. The mucous cover was ſo tranſparent, that I might have ſuppoſed the tadpoles to be naked, if I had not been ſure of the preſence of the gluten. But I could not perceive any pore or aperture either on it or the tadpoles. Neither did a glaſs of more highly magnifying power ſhew any ſuch appearance. I next ſtripped the fetuſes of their mucus, but diſcovering nothing in thoſe globules that were full, I determined to try whether evacuating them would contribute to the ſucceſs of my enquiry. Making, therefore, an inciſion with a ſmall ſcalpel, I preſſed out all the ſemifluid matter lying within, ſo that nothing remained but the empty ſkin or membrane. This was examined firſt with a weak and then with a powerful lens, when there appeared an immenſe quantity of lucid points, which may reaſonably be ſuppoſed to be ſo many pores. I ſaw the ſame apertures upon the tadpoles of toads. This ſuppoſition, therefore, ſhews how the ſeminal fluid enters into the body of the tadpole through innumerable mouths. The preſence of theſe apertures upon every part of the ſkin, explains a phænomenon before mentioned, why fecundation takes place whatever part of the globule, whether that which correſponds to the belly, or the head, or the tail, be touched with ſeed; for when this fluid has traverſed the gluten, it is ſure to meet with paths prepared for its entrance.

CLIII. III. Thoſe, who have ſuppoſed that the fetuſes are not generated during the act of fecundation, but that they pre-exiſt long before, and are contained within the female, have explained their animation, by imagining, that the ſeed of the male enters into the fetus, and reaches the heart. By gently irritating the cavities of this organ, it excites alternate dilatation and contraction, and thus the fluids are forcibly impelled into their reſpective veſſels. Hence follow, as ſo many neceſſary conſequences, the dilatation of the veſſels, an increaſe of the quantity of fluids, a greater irritability of the heart, an univerſal expanſion of the ſolids, and conſequently the growth of the animals both in bulk and maſs. Such is the doctrine delivered among others by Haller and Bonnet in their uſeful productions, and long before them by Valliſneri, who deſerves the greater praiſe, ſince he was unacquainted with the facts that directly prove the pre-exiſtence of the germ, and was guided only by the twilight of conjecture. When I conſider the evident pulſation of the heart, which is previous to the leaſt motion in any of the limbs, the evolution of the veſſels and of the whole animal, which is conſequent upon the action of this muſcle, its irritability,—that property, by virtue of which it reſumes motion, when ſtimulated by any mechanical agent, at a time when there are no remains of irritability in the other muſcles, all theſe circumſtances induce me to adopt the opinion of theſe writers. I ſhall, therefore, with them conſider the ſeminal fluid as a ſtimulus, which penetrating to the heart, and powerfully irritating the internal parts, excites more frequent and ſtronger pulſations; whence ariſe that manifeſt extenſion of parts, and that animation which ever follow impregnation.

Both theſe effects are influenced by the temperature of the atmoſphere. The ſagacious reader eaſily conjectures, that they will ſooner take place in warm weather, and later in cold. Tadpoles fecundated early in the ſpring, as thoſe of the terreſtrial toad with red eyes and dorſal tubercles, require ſeveral days to appear; and if the heat ſhould be only a few degrees above congelation, ten or eleven are neceſſary. On the contrary, the evolution of the tadpoles of the fetid toad and of frogs is very quick, for they are fecundated in May and June. I once obſerved, that when the thermometer ſtood at 21°, they became animated in leſs than twenty-four hours; whereas thoſe of the very ſame ſpecies (viz. of the green aquatic frog) did not arrive at this ſtate till the fifth day, when the temperature of the air was about 13° (*a*). The ſame variation is obſervable in the fetuſes of the water-newt, of ſcaly fiſhes, the tortoiſe, the crocodile, and in general of oviparous inſects: and what is truly ſurpriſing and ſcarce credible, the difference of temperature has the ſame effect upon the eggs of birds, of which it might be imagined, that they could not be hatched but in a determinate heat, ſuch as that of the female, which is about 32° (*b*). In this

(*a*) $61\frac{1}{4}$ of F. (*b*) 32=104 of F.

temperature

temperature the eggs of hens are hatched very regularly in twenty-one days. But by a diminution of the heat, Mr. Villers, a ſenſible naturaliſt of Lyons, has retarded the appearance of the chickens to the 25th; and Mr. D'Arcet, by increaſing the warmth, has hatched them on the 13th day.

CLIV. IV. I have had occaſion to make a ſingular obſervation upon the ſeed of the frog and toad, the proper ſtimulant of the heart of the tadpole. It is diſtinguiſhed by two properties from the ſeminal liquor of other animals. It is as tranſparent as water, and has not the leaſt viſcidity. It evaporates nearly in the ſame time as water, and hence would ſeem to have no ſpirituous parts; as is moreover evident from its not taking fire, when brought to the flame of a candle, and extinguiſhing burning coals when it is thrown upon them. To the taſte it is inſipid, and as it does not efferveſce either with acids or alkalies, it muſt be conſidered as neutral. Theſe qualities would appear to agree but ill with the nature of a ſtimulant, if we underſtand by this term a pungent, cauſtic, or ſpirituous ſubſtance. But this is not the ſenſe in which Haller and his followers conſider thoſe bodies which ſtimulate the heart. They mean ſuch as are capable of irritating the fibres of this muſcle, by which means it is excited to motion, if at reſt, or if otherwiſe to an increaſe of action. To this claſs of ſtimulants belong the blood, water, air, and the ſeminal fluid, which will appear to be more powerful than the fluids previouſly circulating in the tadpole, if we conſider the ſlowneſs of their motion.

CLV. V. A quantity of feed far more inconfiderable than we fhould ever have imagined, is fufficient to animate a tadpole. We have feen, that it is not neceffary to cover the fetus compleatly with this prolific fluid. A drop will fuffice (CXL). Further, three grains mixed with twelve, and even with eighteen ounces of water, communicate to every part of it the power of fecundation, fince tadpoles placed in any part of the mixture are impregnated (CXLII, CXLIII). The three grains of feed muft therefore have been diffufed through the whole mafs of water. But what an enormous divifion of its particles muft fuch a diffufion occafion? How fmall a portion of the prolific liquor will fall to the fhare of each tadpole? Yet there are facts which prove, that the femen ftill retains its virtue after this exceffive divifion; for I have found a globule one-fiftieth of a line in diameter taken out of a mixture of three grains of feed, with eighteen ounces of water, was often capable of fecundating a tadpole (CXLIV). Defirous of knowing the proportion which the tadpole (that of a frog is two-thirds of a line in diameter) bears to the particles of feed diffufed in a drop of this dimenfion, I have found, on calculation, that it is as 106,477,777,7 : 1. How infinitely little, therefore, is the quantity of feed in comparifon of the bulk of the fetus which it fecundates! This deduction led me to calculate the weight of the particles of femen difperfed in this drop of water: it is $\frac{1}{199,468,750,0}$ of a grain. That I might view thefe particles under every poffible afpect, I re-

reduced their bulk to cubic lines, when it appeared to be about equal to $\frac{1}{300,212,042,0,}$ of a cubic line (*a*).

CLVI.

(*a*) This note will ſhew how I obtained theſe numbers. Aſſuming with Metzius, that the circumference of a ſphere is to its diameter as 355 : 113, the ſolid contents of a ſphere $\frac{2}{3}$ of a cubic line in diameter will be $= \frac{8}{27} \times \frac{355}{113 \times 6} = \frac{1420}{9153}$ of a cubic line, and this will be the bulk of a tadpole to be fecundated.

The ſolid contents of a ſphere $\frac{1}{50}$ of a line in diameter are $= \frac{1}{125000} \times \frac{355}{113 \times 6} = \frac{355}{847,500,00}$, and this will be the maſs of the drop of water mixed with ſeed.

The ſolid contents of a cylinder which has 37 lines for the diameter of its baſis, and is 35 lines high, are $= \frac{37 \times 37 \times 35 \times 355}{113 \times 4}$; and this is exactly the volume of water weighing 18 ounces, in which three grains of ſeed have been dropped.

The ſolid contents of a cylinder of which the baſe is one line in diameter, and the height 10 lines, is $= \frac{10 \times 355}{113 \times 4} = \frac{3550}{452}$; and this is the volume of the three grains of ſeed.

Now ſuppoſing that the ſeed is equally diffuſed in the water, the volume of water with the ſeed, and the volume of the drop of water mixed with ſeed, will be proportional to the weight of all the ſeed, and to the weight of the ſeed mixed with the drop of water. Hence, as the weight of all the ſeed is three grains, it will appear from the rule of three, that the weight of the ſeed diffuſed in the drop of water is $= \frac{1}{299,468,750,0}$ of a grain.

As in homogeneous bodies the weight is directly as the bulk, having the weight of the whole ſeed and that of the ſeed mixed with the drop of water, we ſhall find by the

 rule

CLVI. VI. Here I may be aſked, how a portion of ſemen ſo infinitely ſmall can make ſuch an impreſſion upon the heart of a tadpole as to accelerate its pulſations and increaſe their ſtrength, and thus to quicken the motion of the fluids and animate the whole ſyſtem. I would reply, that there are not wanting examples in the animal kingdom to give countenance to ſuch a ſuppoſition. A drop of the poiſon of the viper, introduced into a wound, is ſufficient to deſtroy the irritability of the muſcular and the ſenſibility of the nervous ſyſtem, and to cauſe the death of any animal, whether great or ſmall. A man, a dog, an horſe or an ox, are deſtroyed juſt as certainly as a ſparrow, a goldfinch, or a mouſe. If we conſider for a moment, the proportion between the bulk of the venom and that of an horſe or ox, we ſhall not be leſs ſurprized, that ſo ſmall a quantity of poiſon ſhould be fatal to theſe great animals, than that ſo minute a portion of ſeed ſhould animate one infinitely ſmaller (*b*). But we have facts that more

rule of three, that the bulk of the ſeed contained in the volume of water is nearly $\frac{1}{300,212,042,0}$ of a cubic line.

If we now compare the bulk of the tadpole with that of the ſeed contained in the drop of water, it will appear to be as 106,477,777,7 : 1.

(*b*) I cannot but remark, that the author has been ſomewhat unfortunate in the example which he has choſen for the purpoſe of illuſtration. Fontana has proved, by numerous experiments, that the effects of the venom of the viper are directly as its quantity, and inverſely as the bulk of the poiſoned animal. This is very ſtriking in his trials on dogs, of which the ſmaller ſpecies were always deſtroyed by the ſame quantity that ſometimes killed the middle-more directly prove, how very ſmall a quantity of ſeed may produce a conſiderable irritation of the heart. A grain of ſtorax thrown upon the fire will fill a whole room with its odour. The particles therefore muſt be diſperſed through every part of it, and notwithſtanding their extreme minuteneſs, are capable of vellicating the nerves of the noſe, and exciting the idea of a particular ſmell. This vellication is ſo ſtrong, as to occaſion ſneezing in ſome perſons of the more delicate ſex. If then the inconceivably minute exhalations of certain bodies make ſuch impreſſions upon our organs, why ſhould we wonder that a few particles of ſeed prove a ſtrong ſtimulus to the heart of the tadpole, an organ ſo much more ſuſceptible of external impreſſions, as it is more finely framed than ours?

CLVII. VII. But the quantity of ſeed requiſite for the animation of a tadpole has its limits; it is not infinitely ſmall. I have expreſsly obſerved, that the quantity above ſpecified, was *frequently* ſufficient for the purpoſe (CXLIV, CLV). Whence it is to be inferred, that it ſometimes failed. The diminution of the doſe of ſeed afforded a more deciſive proof. I put a drop of this fluid upon a ſmooth pane of glaſs, and drew it out with the point of a needle, into a fine and almoſt inviſible filament, ſometimes an inch long. I then took ſeveral mucous ſpheres and rolled them ſometimes along half, ſome-

middle-ſized, and ſcarce ever the large ones. From the data afforded by his experiments he calculates, that the poiſon of ſeveral vipers is requiſite to deſtroy the life of a man, and a ſtill larger quantity to kill an ox. T.

times

times along a third, at others along a quarter of this filament. This experiment is analogous to another related in paragraph CXLIII, where I obſerve, that if three grains of ſeed be diluted in two or three pounds of water inſtead of a pound and a half, fewer tadpoles will be evolved, and that the number continues to decreaſe in proportion to the increaſe of the quantity of water. The portion of ſeed then neceſſary to produce fecundation, muſt amount to a determinate quantity.

I am notwithſtanding of opinion, that the quantity of ſeed which effects impregnation is ſmall beyond conception, and nearly approaching to what is ſpecified in the CLVth paragraph. Hence I am inclined to believe, that the ſurplus does not contribute at all to fecundation. My opinion is founded upon the following conſiderations. As fecundation is an indiviſible operation, if the ſuperabundance contribute to promote it, it can only be by haſtening the animation and growth of the tadpoles. This muſt be in conſequence of a greater number of particles exciting the heart to ſtronger vibrations. But ſuch a notion is repugnant to facts; there is no difference either in the time or the degree of evolution, whether the fetuſes be entirely covered with pure ſeed, or only touched with an infinitely ſmall drop (CLXIV). I cannot therefore imagine, what purpoſe the ſurplus of ſeed can ſerve, and am obliged to conſider it as uſeleſs. Nor is it difficult to comprehend, how an inviſible particle only may be capable of producing fecundation, if it be conſidered how inexpreſſibly ſmall is the

the area of the veſſels which it enters, for canals ſo narrow muſt refuſe to admit above a very minute portion.

CLVIII. VIII. But a very obvious queſtion occurs here. As theſe amphibious animals are animated by ſuch a trifling quantity of ſeed, may this diſcovery be extended to fiſhes, birds, quadrupeds and man? Or may it be aſſerted in terms ſtill more comprehenſive, that nature employs only ſo inconſiderable a portion of ſeed for the fecundation of animals in general? The queſtion is curious and intereſting, but cannot I think yet be ſolved for want of data. I am aware, that this opinion is poſſible, nay, that the ſtriking inſtance before us renders it in ſome meaſure probable. I cannot, however, be certain, that it is really ſo, when I reflect upon the multiplicity of means employed by Nature to attain the ſame end. Perhaps, therefore, this ſovereign Artiſt may allot various quantities of ſeed for fecundation, according to various qualities of animals. But in order to diſcover whether ſhe really admits ſuch variety, I can imagine no other way than that of compariſon. Let us, for inſtance, compare what happens in the amphibious animals which are the ſubject of theſe Diſſertations, with what takes place in other claſſes, as in fiſhes, inſects, birds and quadrupeds. The knowledge derived from ſuch different inſtances, may enable us to fix our ideas with ſome degree of ſafety. We have, I believe, but two diſcoveries on artificial fecundation, that of Mr. Jacobi, inſerted Berlin Memoirs (*a*),

(*a*) Tom. 20.

and

and mine. But without any deſign to leſſen the merit of that naturaliſt, I may be allowed to ſay, that from his experiments we can only conclude, that artificial fecundation took place in the two ſpecies of fiſh of which he ſpeaks. Theſe were the ſalmon and trout. After putting their mature, but unimpregnated eggs in clear water, he let fall upon them ſeed from the milt of the male, till the water began to grow white. In about five weeks the young fiſhes made their appearance. This is the amount of the whole paper of Mr. Jacobi. It affords no information concerning the preſent queſtion, ſince the German experimenter has not aſcertained, with any degree of preciſion, the quantity of ſeed neceſſary to produce fecundation. Inſtead of doing this, he has ſtopped at the firſt ſtep, and has finiſhed his trials juſt where an inquiſitive naturaliſt would have begun them.

But if experiments on artificial fecundation are not yet ſufficiently numerous to afford the neceſſary data, we cannot expect them from natural impregnation. A ſmall ſhare of information concerning the generation of man and animals will convince us of this truth. It remains, therefore, that we proſecute the experiments upon artificial fecundation in a rational manner. Beſides the light which a ſeries of well-deviſed trials would caſt upon the problem propoſed, it would certainly clear up many obſcurities in the hiſtory of animals. My enquiries relate to an order of beings, in which we are certain that fecundation is external. But I invite naturaliſts to beſtow part of their attention and ſagacity upon that incomparably more numerous claſs, in which it is equally certain, that this function is performed within them. Of these, some, as is well known, are oviparous, others viviparous. It will be proper to keep some females apart from the males, and to observe the time at which they lay their eggs. We may then make upon the eggs such experiments as are described in this Dissertation. I do not think it will be difficult to adapt them to viviparous animals, if we use different means, and such as will act internally, nor do I see any reason to despair of success. Attempts, apparently more arduous, have not disappointed the hopes of the adventurous enquirer, and such unexpected instances of success have occasioned revolutions in natural philosophy.

CLIX. IX. The seed, agreeably to the principles of Mr. Bonnet, is not only the stimulant, but the nutriment of the fetus. The proofs of his opinion are these. The organ of the voice in that mule, which is the product of the mare impregnated by the ass, very closely resembles that of the male parent. If the germ exist in the female before impregnation, it must have been a horse in miniature, and not a mule or an ass; the organ therefore of the voice ought to correspond to that of the horse and not of the ass. The seed must then have modified the organ of the voice in the germ according to that of the male parent. In conformity with this model, the parts of this organ grow and are unfolded; yet we know the ass contributed only a fluid. It is therefore necessary to admit, that this fluid nourishes the component parts of the larynx, and that it is incorporated with

with their ſubſtance, ſince growth is the natural and immediate effect of nutrition. The ſperm then is not a ſimple ſtimulant, it is moreover a nutritive liquor. The naturaliſt of Geneva ſtrengthens his deduction by the inſtances of the growth of the beard in man, of the creſt of the cock, and the horns of the ſtag, which are certainly owing to the efficacy of the ſeed.

Haller, in his great work, agrees only in part with the before-mentioned philoſopher. He allows that theſe phænomena depend upon the ſeed, not indeed as a nutritive liquor, but as an impelling or ſtimulative principle. The evolution of the hair, the horns, and the creſt, is owing to the ſperm, which, being received again into the blood, excites the heart, by its alcaleſcent acrimony, to attenuate by ſtronger pulſations the viſcid juices, and to impel them into the ſmall veſſels, by which means the preexiſting germs of the hair and the horns pullulate, and the creſt grows to a larger ſize (*a*). With regard to the organ of the voice in the mule, its evolution ariſes from the greater power in the ſeed of the aſs than the horſe, to unfold and enlarge the parts (*b*). It appears, therefore, that according to the Swiſs phyſiologiſt, a ſtimulating quality alone is ſufficient, not only for the animation of the fetus, but alſo to explain thoſe phænomena, which, according to Mr. Bonnet, require, that the ſeed ſhould be conſidered as nutritive. I ſhall not preſume to decide between theſe great philoſophers. I

(*a*) El. Phyſ. l. xxvii. Sect. 3. (*b*) L. c. lib. xxix. Sect. 2.

venerate

venerate their merits, and am better qualified to admire than to judge them. I will only obſerve, with the utmoſt deference, that in the frog and toad the prolific fluid cannot be ſuppoſed with any probability to perform the office of a nutritive principle. I intreat the reader to call to mind what is ſaid in paragraph CLV, about the proportion between the bulk of the ſeed and that of the tadpole; it is nearly as 1 : 106 4777777. Suppoſing, therefore, the tadpole to be nouriſhed by the ſeminal fluid, its growth would not exceed the bulk of the ſeed, that is to ſay, it would be next to none. It may be added, that if the nutrition of the tadpole, at the commencement of evolution, came from the ſeed, thoſe which were bedewed with the greateſt quantity of this fluid, ought to grow faſteſt and be evolved ſooneſt. Yet we know, that a tadpole touched with the moſt inconſiderable drop, and one covered completely with this fluid, do not differ in this reſpect (CXLIV). I therefore, from my experiments, cannot aſſign to the ſeminal liquor any other quality, beſides that of a ſtimulant.

Though the preceding obſervations afford the ſtrongeſt reaſons for believing, that the ſeed is a real ſtimulus to the heart of the tadpole; yet, as this is a mere hypothetical opinion, I was deſirous of ſubmitting it to the teſt of experiment. Reflecting, that the ſtimulating power of any ſubſtance muſt be enhanced by increaſing the activity of its parts, and that this end may be attained by heat, I determined to warm the ſeminal fluid, and then to to moiſten ſome tadpoles with it. With this view I mixed three grains of the ſeed

ſeed of the frog with half an ounce of water, and having heated it to 30° (*a*), I immerſed twenty tadpoles in it, and immediately afterwards removed them into pure water, in which the thermometer ſtood at 17°. That I might have a term a compariſon, I fecundated other twenty tadpoles with the ſame mixture, after it had acquired the temperature of the atmoſphere. The device was not ineffectual. The tadpoles fecundated in the heated mixture, were ten hours more forward than the others, a phænomenon which I cannot but attribute to the greater energy and ſtronger ſtimulating power ariſing from the action of heat.

CLX. X. The ſtimulating power, which animates the embryo, is not loſt, though the ſeed ſhould have been kept ſome time after it has been taken out of the body. This happens alike, whether it be kept unadulterated or mixed with other liquors (CXXXI, CXLV). It is truly wonderful that a ſingle drop ſhould not be loſt, when mixed with ſo large a body of water (CXLII, CXLIII). Will the ſeminal liquor of other animals preſerve in like manner its efficacy? For want of facts we muſt have recourſe to conjecture. The preſent inſtance may be an exception to a general rule and confined to a particular claſs, in which fecundation is external and takes place in water, and on this account the ſeed is expoſed to circumſtances, never experienced by that of other animals. But, on the other hand, it is poſſible that thoſe properties may belong to other ſeminal fluids. In my *ſecond Eſſay on the Na-*

(*a*) $30° = 99\frac{1}{2}$, and $17 = 70\frac{1}{4}$ of F.

tural

tural Hiſtory of Animals and Vegetables I obſerve, that the ſeed of man, and various quadrupeds, does not become corrupt for ſeveral hours after it has been taken from its natural receptacles. I confirm this obſervation by ſhewing, that the worms continue to ſwim about briſkly for ſome time, provided it is kept properly heated. If, therefore, the ſeed of man and quadrupeds preſerve its natural qualities under theſe circumſtances, why may it not alſo retain its ſtimulating power, which is the cauſe of fecundation? And if ſo, is the famous ſtory of Averroes, concerning that unfortunate queen, who became pregnant after bathing, without any commerce with man, ſo very ridiculous?

CHAP. V.

WHETHER FECUNDATION IS AN EFFECT OF THE AURA SEMINALIS. WHETHER OTHER LIQUORS ARE CAPABLE OF PRODUCING FECUNDATION. TRIALS TO PROCURE ARTIFICIAL MULES IN THE AMPHIBIOUS ANIMALS IN QUESTION. ARTIFICIAL FECUNDATION OF THE SILK-WORM. ATTEMPT TO IMPREGNATE A BITCH ARTIFICIALLY.

CLXI. WHETHER the groſs viſible part of the ſeed be neceſſary to the fecundation of man and animals, or, whether the inviſible attenuated part, uſually called the *ſeminal vapour* or *aura*, be deſtined to this purpoſe, is a very ancient queſtion, and ſtill continues to be debated every day. Thoſe phyſicians and philoſophers who favour the latter opinion, are obliged to maintain it from a ſort of apparent neceſſity, rather than by any direct reaſons or experiments. They reflect upon obſervations made by diligent anatomiſts, who have found the *vagina* of ſome pregnant women either very narrow or entirely cloſed. They attend alſo to other numerous obſervations, from which it would appear, that at the time of impregnation, the ſeed does not reach the uterus. Laſtly they reflect, that the orifice of the oviducts, or Fallopian tubes, is ſo narrow, that it will ſcarce admit air, much leſs a probe. They conclude, that ſo many obſtacles muſt prevent the ſeed injected into the female organs from arriving at the ovarium, where the embryos are lodged. Hence it is ſuppoſed, that fecundation muſt be effected by the volatile part of that liquor, which either reaches the ovarium by the way of circulation, or by riſing through the mouth of the uterus and along the tubes. Notwithſtanding theſe arguments, many other authors adopt the oppoſite opinion; they think, that the groſs part of the ſeed operates impregnation, ſince the narrow paſſages are enlarged during the ardour of enjoyment, and ſince there are not wanting inſtances where ſeed has been found in the uterus, in the tubes, and even to have aſcended as high as the ovaria. That the uterus ſhould often, after coition, be found without the fluid of the male, they conſider as an objection of no weight, becauſe caufe the obfervation was probably made after too long an interval, when the feminal liquor had paffed out of this cavity, or becaufe fo fmall a quantity had entered into it as to efcape the notice of the infpector.

Such nearly are the arguments on each fide, but they are, in my opinion, infufficient to decide the difpute; the former, becaufe they do not irrefragably prove, that the volatile vapour alone reaches the ovaria; the latter, becaufe though the grofs part fhould arrive there, it ftill remains doubtful whether the animation of the embryo is effected by this, or the attenuated part. To cut fhort all controverfy, it will be neceffary to feparate the aura, and to fubject the fetus to its influence, for it will either be impregnated, a manifeft proof in favour of the feminal exhalations, or it will not, and then we may conclude, with equal certainty, that the fenfible part is neceffary to fecundation. But this mode of deciding the queftion has not, I believe, occurred to any one, or at leaft I do not know that any one has reduced it to practice. But from the facility of making experiments with the feed of the animals concerning which I have been treating, this appeared eafy to be done.

CLXII. The reader will recollect what has been before faid of the efficacy of feed, after it has been almoft infinitely divided. A drop of water, one-fiftieth of a line in diameter, taken from eighteen ounces of water, with which three grains of feed are mixed, is capable of impregnating a tadpole (CXLIV). This experiment is apparently favourable to the feminal aura, which, in the general opinion, is nothing but the vapour of the ſeed exceedingly rarefied. The facts, however, which I ſhall adduce, clearly prove the contrary. That I might irrorate tadpoles abundantly with this exhalation, I put a quantity of ſeed, amounting to eleven grains, taken from ſeveral fetid toads, into a watch-glaſs. In another, ſomewhat ſmaller, I placed twenty-ſix tadpoles, which were fixed very firmly to the bottom by the tenacity of the gluten. I then inverted it over the other, and in this ſituation both glaſſes were left five hours in my apartment, where the liquor of the thermometer ſtood at 18°. The ſemen lay exactly under the tadpoles, and they could not but be involved in the riſing vapours, for the diſtance was little more than a line. Upon inſpecting the tadpoles at the expiration of the fifth hour, I found them ſo covered with moiſture, as to wet my finger when I touched them; the moiſture was the evaporated part of the ſeed, which had loſt one grain and half. The tadpoles, therefore, had been bedewed with one grain and half of the ſeminal aura, for it cannot be ſuppoſed to have eſcaped out of the glaſſes, they fitted ſo cloſely. The tadpoles, notwithſtanding, being placed immediately in water, and left there ſeveral days, all periſhed.

CLXIII. Though this experiment is unfavourable to the aura, yet it ſtood alone, and I could not avoid further enquiry. One grain and half ought indeed to fecundate many thouſands, much more twenty-ſix. I however choſe to increaſe the quantity of vapour, which could be done by only increaſing the heat. Placing every thing exactly as in the former

former experiment, I ſet the glaſſes in the window, where the heat of the ſun, being moderated by the glaſs through which it ſhone, amounted to 25°, and could not therefore be prejudicial to fecundation. In four hours the ſpheres were ſo covered by the vapour, that drops were ſeen hanging from them. But the effect produced was the ſame as before.

I repeated this experiment once more, not ſo much with a view of confirming the former reſults, as to ſee whether the ſeed, after part had been reſolved into vapour, retained its efficacy. After the tadpoles were moiſtened with the exhalation, half of them were put, as before, into water, all which came to nothing. The other half were bedewed with a little of the reſiduum, and then put into water: they almoſt all came to perfection. Theſe experiments ſhew, that the vapour of the ſeed of the fetid toad is incapable of impregnating the young, and that the ſeed, after a conſiderable evaporation has taken place, is ſtill efficacious.

CLXIV. Both theſe conſequences were confirmed by ſubſequent experiments. The ſpace between the tadpoles and the ſperm was about a line: that the vapour might be more efficacious, I reduced it to one-third of a line; but ſtill to no purpoſe.

I have already obſerved, that by the aura ſeminalis the vapour of the ſeed is generally underſtood. Some phyſiologiſts think, that this exhalation conſiſts of the odoriferous particles of that fluid; others that it is the moſt attenuated part, and others again that it is a very ſubtle ſpirit. Whatever it be, it is certainly incapable of producing fecundation.

Yet as ſo ſubtle a ſpirit might be thought to eſcape at the meeting of the glaſſes, I determined to obviate this ſuſpicion by cementing the edge of the upper glaſs to the inſide of the other. I moreover ſubſtituted a little glaſs-funnel in the room of the upper watch-glaſs, and cemented it as before; the ſmaller orifice was hermetically ſealed. The tadpoles were faſtened to the neck, and the ſurface of the ſeed was enlarged that evaporation might be quicker. I thought that the conical form of the funnel would collect the aura into the point where the tadpoles lay. This new apparatus was kept ſix hours in a heat of 26°, and the fetuſes were ſurrounded continually by the vapour, but the event was ſtill the ſame, and the reſiduum was alſo efficacious (CLXIII).

When I tried the effects of the aura in open veſſels, where the air had free acceſs, I found it juſt as ineffectual as ever.

CLXV. My laſt experiment made with this view was to collect ſeveral grains of the evaporated fluid. I then immerſed twelve tadpoles in it, and left them ſeveral minutes. Twelve more were touched with the reſiduum, which did not exceed half a grain, but eleven of theſe grew to perfection, but not one of the others.

Theſe various facts concur to prove, that fecundation in the fetid toad is not the effect of the aura ſeminalis, but of the perceptible part of the ſeed. But my enquiries have been extended further; the abundance of the toad with red eyes and dorſal tubercles, and of the green aquatic frog, has afforded me ample opportunities of repeating upon them the experiments deſcribed in paragraphs CLXII, CLXIII, CLXIV, CLXV. But the aura appeared on all occaſions incapable of producing fecundation. My few trials upon the tree-frog agree exactly with the others.

CLXVI. As I had found the juice of the teſticles equally powerful with the ſeed, it was proper to make trial of its aura. I had uſed it with equal advantage, when pure and when diluted with water. I have evaporated it under both theſe circumſtances in the ſame manner as ſeed; but always to no purpoſe: whence it was neceſſary to conclude, that this aura is juſt as inert as that of the ſeed.

My experiments on theſe two ſpecies of aura were made in the ſpring and ſummer of 1777, and I then gave intimation of them to Mr. Bonnet, as may be ſeen by ſome extracts of my letters, inſerted in the uſeful annotations on the third volume of the new edition of his works. I repeated them in 1780, with the ſame reſult.

Thus then it appears, that in two ſpecies of the toad, and as many of the frog, fecundation is not the effect of the aura, but of the groſs part of the ſeed. But are we to ſuppoſe that Nature obſerves this rule in animals and man? Sound logic does not allow us to deduce from ſo few facts a concluſion ſo general. But theſe facts lead us to think the ſuppoſition probable. It is at leaſt certain, that we may venture to admit it, till contrary facts ſhall be adduced. And thus the great queſtion, whether fecundation is the effect of the aura ſeminalis, is clearly decided in the

 negative,

negative, with reſpect to ſome animals, and with probability with reſpect to the reſt.

CLXVII. I may here obſerve, that it appears from this, as well as the preceding chapters, that the part of the ſeed which is capable of effecting impregnation is not, as many ſuppoſe, a ſpirituous liquor, of great volatility, and liable to loſe its virtue when expoſed for ſome time to the air, after which there remains nothing but an effete matter, a kind of caput mortuum, as is incident to many liquors, produced both by art and nature, which, on this account, are kept carefully ſhut up in veſſels. But nothing like this happens to the ſeminal fluid, though it be expoſed to the air for ſeveral hours; and after half had been driven off in vapour, the remainder was juſt as fit for fecundation as that which had been recently taken from the veſicles. The prolific liquor, to which theſe obſervations are applicable, may be compared in ſome meaſure to water. Whether this element evaporates, either from agitation of the air, or the action of heat, the reſiduum retains the nature and properties of the whole maſs. But we may, on the other hand, remark a great difference. The vapour of water, when collected, poſſeſſes every property of water never ſubmitted to this proceſs, whereas the vapour of ſeed, though it may be converted into a liquid, does not retain its fecundating power. It muſt, therefore, be ſuppoſed, that the particles, when ſeparated from the whole maſs, acquire ſome bad quality, which diſqualifies them for occaſioning ſuch an irritation of the heart of the embryo, as will produce animation;

though

though the minuteneſs of the particles does not allow us to inveſtigate the origin and nature of this bad quality.

CLXVIII. I now proceed to the diſcuſſion of another problem, which will appear both new and extraordinary. It was propoſed by Mr. Bonnet, in a letter concerning artificial fecundation. And although he was aware of its extravagance, he did not heſitate to communicate it, in order that it might be put to trial. I cannot explain his idea better, than by laying before the reader an extract from his letter of the 15th of Auguſt, 1778. "I will not conceal from you a viſion that has found its way into my head. Mr. Senebier muſt have informed you of the fine experiment of Mr. Achard, who has ſubſtituted electricity, in the room of heat, for hatching chickens, and has ſucceeded at leaſt in part. If the electrical fluid be capable of cauſing the evolution of the chicken in the egg, it muſt be owing to an acceleration of the velocity of the fluids, or, what amounts to the ſame thing, to an increaſe of the irritability of the heart. Now I think I have given ample proof, that the ſeminal liquor fecundates the germ, by exciting the irritability of the heart. I could wiſh then, my dear Malpighi, that you would ſubſtitute the electrical fluid in the ſtead of the ſemen. Should an experiment ſo original ſucceed, the fecundation would be far more artificial than that which you have ſo happily effected. You may well imagine that I will not anſwer for your ſucceſs: it is probable, that the electrical fluid cannot perform the office of the ſeminal liquor; but we have ſeen ſo many unexpected

events

events happen in the organized kingdom, that we cannot pronounce with too much caution concerning the impoſſibility of any experiment, more eſpecially of ſuch as relate to this ſubject.—Should we have ſuſpected, that the polype poſſeſſes ſuch ſurprizing properties? And after the diſcovery of the polype, who would have ſuſpected the reproduction of the head of the ſnail?"

Such were the expreſſions of this profound contemplator of Nature. But various occupations of another nature having prevented me from continuing my obſervations on artificial fecundation, it was not in my power to attempt this ſtrange experiment, till the ſummer of the preſent year (1780). To confeſs the truth, I doubted exceedingly of its ſucceſs, not ſo much for the reaſons adduced by my celebrated friend, as on account of the ſtriking difference between Mr. Achard's eggs, which had received the influence of the male beforehand, and the tadpoles which had not been ſubjected to it.

Having placed upon the conductor a metallic veſſel, containing ſeveral fetuſes both of the frog and toad, I excited the cylinder, and ſimply electrified them without ſparks or ſhocks. In the firſt trial, the tadpoles were electrified three hours a day for two days ſucceſſively. As theſe became all putrid, I continued the proceſs in another experiment for four hours on three ſucceſſive days, but with no greater ſucceſs. I then electrified nine tadpoles for thirty-three hours in the ſpace of two days and a half, but the event was ſtill the ſame.

Inſtead

Inftead of placing the tadpoles in the fituation already defcribed, I fixed them to the point of a metallic rod arifing from the conductor, knowing that the electrical fluid would now be more concentrated and energetic. But I was not more fuccefsful, though they were electrified nineteen hours in two days. All thefe experiments were made in hot weather, and the tadpoles fhewed figns of decompofition in three days.

At the fame time I electrified other impregnated tadpoles: they grew fafter than thofe which were not electrified, an event that exactly correfponds with the acceleration of vegetation, occafioned by the electric fluid. From thefe experiments we may conclude, that electricity increafes the velocity of circulation in fecundated tadpoles, but not in the unimpregnated, probably becaufe its action is not fo mild and gentle as is neceffary at this early period, and as that of the feminal liquor.

CLXIX. Before Mr. Bonnet imparted to me his ingenious thought, and indeed at the time of my firft obfervations concerning artificial fecundation, I conceived hopes of being one day the fortunate difcoverer of fome fluid, equally efficacious with the femen, though of a different nature. When I was employed about the impregnation of the toad with red eyes and dorfal tubercles, and had difcovered the efficacy of the feed taken out of the veficles, I was ftruck, I know not how, by the thought of trying fome of the other fluids of the fame animal. I therefore moiftened fome tadpoles with blood, others with gall, others with juice expreffed expreſſed from the different viſcera, as the heart, the liver, the lungs; they were put into ſéparate veſſels, and ſome of thoſe that had been touched with blood and the juice of the heart, came to perfection. The ſurprize, which an event ſo unexpected occaſioned, may be eaſily conceived; but it may likewiſe be ſuppoſed that I ſhould repeat this experiment. But the reſult was now very different; not a tadpole was evolved. The ſame ill ſucceſs attended many ſubſequent repetitions; and I was obliged to conclude, that neither the blood, nor the expreſſed juice of the heart, has any prolific power. But whence, I ſhall doubtleſs be aſked, the ſucceſs of the firſt experiment? It aroſe from an inadvertency which I afterwards eaſily detected. The tadpoles were taken from an uterus, which I had touched with forceps, that had been uſed for taking ſome drops of ſeed out of the veſicles. Not being at that time aware of the efficacy of an exceedingly minute quantity of ſeed, I had not wiped the inſtrument very carefully. On this account, it muſt have left an imperceptible portion upon ſome of the tadpoles, that were afterwards moiſtened with blood and the juice of the heart. This explanation became more probable, when I found, that of tadpoles taken out of the uterus with forceps, careleſsly wiped after they had been dipped in ſeed, a certain number was always impregnated; whereas this never happened, when the forceps had not touched this fluid, or when they had been diligently cleaned. This negligence, and the detection of its origin, were excellent preſervatives againſt the commiſſion

of

of like miſtakes in future. They may alſo ſerve as warnings to thoſe who ſhall be deſirous of repeating my experiments on artificial fecundation, or of attempting others of the ſame nature.

CLXX. Convinced of the inefficacy of theſe liquors, I laid aſide at that time all thoughts of trying others. My unſucceſsful experiments with the electrical fluid, renewed the ſame train of ideas; but it was rather leſt I ſhould have reaſon to reproach myſelf for having too ſoon quitted a purſuit entirely new, than with the hope of ſucceſs, that I reſumed theſe experiments. Guided by the great principle, that fecundation is the conſequence of irritating the heart by the ſeminal liquor, I was led to try acrid and ſtimulating fluids, ſuch as vinegar, diluted ſpirit of wine and urine; but theſe ſubſtances haſtened the corruption of the tadpoles, inſtead of fecundating them. The juice of the lemon and citron produced the ſame effects, though their acidity was weakened by the admixture of water. In the rind of theſe fruits there is a ſtimulating ſpirit, which, upon ſqueezing, ſpirts out in little jets, and takes fire when thrown into the flame of a candle. But this fluid, as well as many others, which I ſhall forbear to enumerate, was totally inefficacious.

CLXXI. We learn, both from ancient and modern hiſtory, that the ſeed of one ſpecies of animal, frequently impregnates the embryos of another, when there is a near reſemblance between them. A third ſpecies, uſually denominated mules, is the fruit of this impregnation. Thus the male goldfinch

produces

produces with the female Canary bird, an intermediate ſpecies; as alſo does the white peacock with the common ſort; and the pheaſant with the hen. Among quadrupeds, the mules, which are the offsprings of the aſs and the mare, or the horſe and the ſhe-aſs, are the moſt common. And the celebrated Bourgelat has now eſtabliſhed, beyond a doubt, that the wolf propagates with the bitch. Nor is it always neceſſary, that the two ſpecies ſhould be very nearly allied. For wherein conſiſts the cloſe reſemblance between the aſs and the cow, or between the bull, the ſhe-aſs, and the mare? Yet the recent and certain obſervations of the ſame French Naturaliſt, have proved, that the ſingular ſpecies of animal called the *Jumart* (*a*), is produced by the copulation of theſe widely differing quadrupeds, after its exiſtence had been queſtioned by modern writers, and expreſly denied by Buffon (*b*).

Theſe inſtances led me to entertain hopes, that I ſhould obtain intermediate productions by impregnating the embryos of one ſpecies with the prolific liquor of another. In my Proſpectus I propoſed to enquire, what would be the event of attempting ſuch experiments on aquatic and amphibious animals, bearing little reſemblance to each other (*c*). If they ſhould happen to be ſucceſsful, it is obvious, how much ſuch irregular productions would contribute to illuſtrate the obſcure function of generation.

(*a*) Œuvres des Charles Bonnet, Tom. V. (*b*) Hiſt. Nat. Tom. XIV. (*c*) P. 58.

I have

I have profecuted this inquiry, but with fmall fuccefs. Having repeatedly moiftened the embryos of frogs and toads with the feminal fluid of the newt, and reciprocally, evolution has not taken place in any one inftance. My hopes of impregnating neighbouring fpecies with effect were much more fanguine. I need not obferve, that the frog and toad are each amphibious, and that they nearly refemble one another, as well in external appearance as in internal ftructure. Befides this correfpondence of organization, their manners, actions, and ways of propagation are alike. Having, therefore, fixed upon the fetid terreftrial toad, as its feafon of generation coincides with that of frogs, I bathed with the feed of this toad, the tadpoles of the green aquatic and the tree frog, and in return with their feed, I moiftened the tadpoles of the fetid toad. I did the fame with the frogs juft mentioned, and the toad defcribed in the CXXVIth paragraph. The feed, as well as the juice of the tefticles, was employed fometimes pure, and fometimes diluted with water. But notwithftanding fo many and fuch ftriking marks of refemblance, one fpecies could not be fecundated by another. If I did not wonder, that liquors effentially differing from feed were not prolific (CLXIX, CLXX), I muft own my furprize at finding that fpermatic liquors, which muft needs refemble each other very clofely, were inefficacious. Thefe experiments, however, though ineffectual, are not uninftructive; they teach us, that from analogy we cannot learn when we may procure intermediate productions. In general we obferve, that they owe their

their origin to animals, in many reſpects, analogous to each other; but ſuch analogy is not a certain ſign of their being capable of producing together, as manifeſtly appears from experiment. In this branch of phyſics, as in numberleſs others, we muſt not generalize our ideas, but are under the neceſſity of conſulting the oracles of Nature, and receiving her anſwers with reſpect and attention, as we proceed from one ſpecies to another. If we would obtain illegitimate productions, we muſt of neceſſity employ the ſeed of a different ſpecies. It muſt, at the ſame time, be capable of penetrating the embryo, and animating it by a bland and kindly impulſe. But we can learn from the effects alone, whether any given ſeed is endowed with theſe eſſential properties, and we muſt abide ſtrictly by the reſult of experiments.

And as if theſe amphibious animals knew the inefficacy of their reſpective ſeminal fluids, I have never, in the whole courſe of my inquiries, ſeen them coupled. At the ſeaſon of their amorous ardour, I have placed a male toad along with a female of his own ſpecies, and a female frog; when diſregarding the latter, he has flown to embrace the former. I have then ſeparated them, and taken away the female: but the male ſhewed no deſire of approaching the female frog, and ſeemed to be wholly bent on eſcaping. And though I kept them together for ſeveral days, copulation did not take place, not even when the female began to diſcharge her young, a time at which the males are more than commonly ardent, in order to fecundate the fetuſes as they

they are brought forth. I witneſſed the ſame inattention in the male of the fetid toad, to the female of the green aquatic and tree-frog, and in the male of the green and tree-frog, for the female of the fetid toad. I know that male toads are commonly ſuppoſed to copulate with frogs, and that many, for this reaſon, abſtain from eating the latter during their amours. I, however, have never met with ſuch conjunctions in the whole courſe of obſervations; nor did Roeſel ever ſee an inſtance in Germany. I muſt, therefore, reckon this opinion among thoſe numberleſs prejudices, which have credulity and ill-grounded popular tradition for their only ſupport.

CLXXII. After having finiſhed the foregoing paragraph, with which I intended to conclude the Diſſertation, I read over the whole from the beginning, in order that I might correct ſuch paſſages as required correction. The CLVIIIth paragraph induced me to attempt another experiment. I there invite naturaliſts to try to fecundate ſome of thoſe animals, in which it is certain, that impregnation takes place internally. It was now July, the ſeaſon deſtined for the amours of the ſilk-worm (*a*), in the Modaneſe, and indeed in moſt other parts of Italy. I therefore determined to try, whether any portion of my ſucceſs, with different amphibious animals, would attend experiments on this inſect. I recollected, indeed, the failure of the great Malphigi; nor can it be doubted,

(*a*) Phalæna Mori. Linn. Syſt. Nat.

that ſuch an inſtance may juſtly create diffidence in the boldeſt experimenter; more eſpecially as the celebrated Bibiena was not more fortunate, when he undertook to repeat and vary Malphigi's experiment. As I did not, however, perceive, that it was impoſſible to ſucceed, I did not think the attempt would argue any blameable preſumption in me. The unimpregnated eggs of the phalæna originating from the ſilk-worm, may be obtained in two ways. If the female be kept apart from the male, they may be collected after they are diſcharged, or they may be taken from the matrix. The ſeed is to be taken from the genital organs of the male. Having procured ſome eggs in both theſe ways, I moiſtened ſome of them with a large quantity of ſeed, and others very ſparingly, but to no purpoſe. The fecundated eggs of this inſect aſſume a violet colour; the others remain yellow. My eggs, before they were moiſtened with ſeed, were of this colour, and retained it afterwards; they moreover became ſoft, and an incavation appeared upon the ſurface, ſigns that never fail to attend ſterility. Theſe experiments were made with that ſpecies, of which the eggs are hatched only once a year, viz. in the ſpring. And I believe, it was upon this ſpecies, that the great naturaliſt of Bologna made his unſucceſsful experiment.

I afterwards tried another ſpecies, which is much cultivated in the cities of Lombardy, on account of its producing three generations in a year; one towards the end of the ſpring, another in ſummer, and the third in autumn.

My

My labour was not unprofitably beſtowed upon the eggs of this ſpecies. Many which I moiſtened with ſeed, produced worms in proper ſeaſon; nor is there any cauſe to apprehend, leſt I ſhould have committed ſome inadvertence in my experiments. As ſoon as the laſt change of form had taken place, I put the females under the receiver of an air-pump; the males were on the outſide; and either the ſight or the ſmell of the females allured them to the veſſel, for they were conſtantly fluttering about it. By theſe means, I ſuppoſed that the male organs of generation would be filled with ſeed. As ſoon as the eggs were diſcharged, I bathed them with ſeminal fluid. Many of them, which were at firſt yellow, began in a few days to turn brown, and at length aſſumed a browniſh violet hue; in about a week they produced little worms; thoſe which had not been moiſtened with ſeed remained yellow, grew flaccid, and ſpoiled. I procured fifty-ſeven at two trials.

CLXXIII. This unexpected event gave me courage to attempt another experiment. It ſhewed, that oviparous animals may be artificially fecundated. Of viviparous animals I had before obſerved with wonder, the artificial impregnation in ſome ſpecies in which this function is external. It remained therefore, as the reader will eaſily gueſs, to try whether ſuch experiments will ſucceed with thoſe viviparous animals in which fecundation takes place in the body of the female; an animal of ſome ſize, ſuch as the cat, the dog, or ſheep, ſeemed moſt fit for my purpoſe.

pofe. This idea had been for fome time fermenting in my head, and I could not refrain from difclofing it in an article written by me for the *Profpectus of the new Italian Encyclopædia*, and entitled, *Artificial Fecundation*. Having propofed fome views refpecting the artificial fecundation as well of plants as animals, I employed towards the end the following expreffions. "Hitherto I have fpoken only of animals ufually called oviparous; artificial fecundation may perhaps, by different means, and fuch as would act within the body, be extended to viviparous animals. The reader underftands my meaning." In the CLVIIIth paragraph of this Differtation I threw out the fame idea, and exhorted Naturalifts to put it in practice; for after I had fucceeded fo eafily in impregnating different animals of another kind by art, I could not confider my project as very unpromifing. The event of my experiments on filk-worms, in which impregnation is internal, rendered my expectation ftill more fanguine, and I immediately fet about to bring them to an iffue. I chofe a bitch fpaniel of moderate fize (*a*) which had before had whelps. Sufpecting, from certain appearances, that fhe would foon be in heat, I confined her in an apartment, where fhe continued a long time, as will be feen below. For greater fecurity, that fhe might never be let loofe, I fed her myfelf, and kept the key the whole time. On the thirteenth day fhe be-

(*a*) Canis aquaticus, pilo longo crifpo, inftar ovis. Linn. Syft. Nat.

gan

gan to ſhew evident ſigns of being in heat; the external parts of generation were tumid, and a thin ſtream of blood flowed from them. On the twenty-third day ſhe ſeemed fit for the admiſſion of the male, and I attempted to fecundate her artificially in the following manner. A young dog of the ſame breed furniſhed me, by a ſpontaneous emiſſion, with nineteen grains of ſeed, which were immediately injected into the matrix, by means of a ſmall ſyringe introduced into the vagina. As the natural heat of the ſeed in animals of warm blood may be a condition neceſſary to render fecundation efficacious, I had taken care to give the ſyringe the degree of heat which man and dogs are found to poſſeſs, which is about 30° (*a*). Two days after the injection, the bitch went off her heat, and in twenty days her belly appeared ſwoln, which induced me to ſet her at liberty on the twenty-ſixth. Meanwhile the ſwelling of the belly increaſed; and ſixty-two days after the injection of the ſeed, the bitch brought forth three lively whelps, two male and one female, reſembling in colour and ſhape not the bitch only, but the dog alſo from which the ſeed had been taken. Thus did I ſucceed in fecundating this quadruped; and I can truly ſay, that I never received greater pleaſure upon any occaſion, ſince I cultivated experimental philoſophy.

CLXXIV. I have before obſerved, that nineteen grains of ſeed were injected; but it is proper to add, that the whole quantity

(*a*) 99 or 100 of F.

did not paſs into the uterus. Thirteen at moſt arrived there, ſince ſix at leaſt adhered to the ſides of the ſyringe. But we are not to conclude, that impregnation was effected by all of them. It ſeems beyond diſpute, that a much ſmaller quantity is ſufficient. As the effect is produced in the ovarium, the ſeed muſt paſs through the tubes, and who does not perceive, that ſome muſt be retained by their ſides, as alſo by thoſe of the uterus? A very ſmall portion muſt therefore, in this caſe, have produced fecundation. Combining this experiment with thoſe made upon amphibious animals, in which we have ſeen how ſmall a quantity of ſeed ſuffices, it may with great probability be inferred, that the doſe of that vital liquid, by which Nature is renewed, is always exceedingly minute, as well in the large as ſmall animals. This inference is ſtill further confirmed by obſervations relative to birds; from which we learn, that a cock fecundates at once all the eggs laid by the hen in twenty days. Hence, as one is ſufficient for twelve or even twenty hens, he may every day be the father of thirty chickens (*a*).

I conclude with a brief reflection. Conſidering my laſt diſcovery, I have no difficulty in believing, that we ſhall be able to give birth to ſome large animals, without the concurrence of the two ſexes, provided we have recourſe to the ſimple mechanical device employed by me; and, at the ſame time, take advantage of ſuch favourable cir-

(*a*) Buffon, hiſtoire des oiſeaux.

cumſtances as may promote the experiment, and uſe thoſe judicious precautions which are indiſpenſible. Meanwhile, reflecting upon the phænomena which I have related, I am inclined to exclaim with Pliny, *Mihi intuenti ſæpe perſuaſit rerum Natura nihil incredibile exiſtimare de eâ.*

END of DISSERTATION II.

TWO LETTERS FROM MR. BONNET, OF GENEVA,

Refpecting the foregoing

DISSERTATIONS.

ARGUMENT.

A LITTLE before I delivered to the Printer the two Differtations on the Generation and artificial Fecundation of different animals, I tranfmitted a French tranflation of the analytical index to my illuftrious friend Mr. Bonnet; for my numerous engagements did not allow me to fend him an abridgment of the Differtations, notwithftanding I, as well as he, very much wifhed it. The great brevity of my communication produced effects, equally advantageous to the public and myfelf, but fuch as I did not at all expect. From its very nature, the index could only ferve to convey a fummary of the contents, and was merely calculated to induce the reader to confult the work itfelf.

ſelf. When I conſidered that the Diſſertations would ſoon appear, I ſuppoſed that my friend would be ſatisfied with the imperfect account I ſent to him. But I was miſtaken. The peruſal of the index excited a deſire of fuller information concerning ſeveral particulars: and he wrote a long and obliging letter, which drew from me an anſwer, in which I endeavoured to reply to the ſeveral pertinent queſtions propoſed by Mr. Bonnet. My anſwer was followed by a ſecond letter, not leſs polite and intereſting than the former. Such was the origin of theſe two letters. I conſider it as a ſingular advantage, that I am authorized to publiſh them; not merely on account of their cloſe connection with the Diſſertations, but alſo on account of their good ſenſe, the philoſophical views, and profound reflections which they contain; and becauſe they are recommended by that elegance (if I may be allowed to judge concerning what is written in a foreign language) which ever characteriſes the productions of this writer.

In the firſt, thoſe paſſages of the analytical index, which require explanation, are frequently tranſcribed. It would have been ſuperfluous to publiſh my reply, containing theſe explanations, as I took it from my book: there the reader will find them, by having recourſe to the numbers of the letter, which correſpond to thoſe of the Diſſertation. With this help, he will alſo enter better into the reaſoning and reflections of Mr. Bonnet upon thoſe paſſages of the index, of which he did not aſk for any explanation.

The

The ſecond letter, which relates to my explanations, is in general very perſpicuous. In thoſe places, both of this and the firſt, which ſeemed to require ſome previous information, I have placed a few notes at the bottom of the page.

LETTER I.

Genthod, Nov. 29th, 1780.

My dear Friend,

I WAS going to write to you at the time I received your intereſting letter of the 7th, for which I return you many thanks. My health, it is true, has been very much diſordered this year. In the ſummer, I had two tedious catarrhal fevers, the one immediately ſucceeding the other; they harraſſed me exceedingly, and very much affected my weak eyes. The violent and repeated efforts of coughing, drove the blood into them in too great abundance. I could do nothing all July and Auguſt and part of September. When I was firſt attacked, I was very buſy about my numerous annotations on the *Contemplation*. I had gone as far as the tenth part. I could not reſume this employment immediately upon my recovery; it required too much application for my weak ſtate. I therefore ſet about the reviſal of the different memoirs

memoirs I had publiſhed in Rozier's *Journal.* Hence I was led to compoſe new ones on bees, ſnails, and newts. They will compoſe the Fifth Volume of the edition in Quarto; and that the preſs may not remain unemployed, I have allowed my editors to print them as they are ſent. The firſt volume of the *Contemplation* was printed, when they began the fifth volume. The *Contemplation* will take up the fourth volume. It will be enlarged about one-third. Hence it became neceſſary to divide the volume into two parts. It will occupy three volumes in octavo. You will eaſily believe, that your name frequently occurs, and I am always highly gratified, when I have occaſion to mention your fine diſcoveries. But I have great reaſon to regret, that I did not ſooner receive the ſynopſis of the new experiments you have obligingly communicated to me. As I treat in the ſeventh and eighth parts of the reproduction of animated beings, I cannot, as thoſe parts are already printed, introduce many curious particulars noticed in your index. But I foreſee, it will not be difficult to introduce many of your obſervations into the chapters not yet printed. I will, therefore, go over the principal articles, obſerving the order of your numbers.

I. I perceive that you have found, by many experiments, that the fetus exiſts before fecundation in the green aquatic frog, the tree-frog, the great terreſtrial toad, with red eyes and dorſal tubercles, the fetid terreſtrial toad, and in two ſpecies of the water-newts (*a*).

(*a*) N° I. of this and the ſecond letter, are the only Numbers which have none to correſpond with them in the Diſſertations.

This

This ſhort liſt will doubtleſs be enlarged, as ſoon as your curious experiments are extended to other ſpecies; and you will always have the diſtinguiſhed merit, of having laid open ſo rich and beautiful a career. All direct means of proving the pre-exiſtence of the embryo in birds and quadrupeds, have by no means been employed. I, you know, have never doubted of this pre-exiſtence: all my reflections upon generation, even in my youth, led me to conſider it as the moſt univerſal law of Nature. To contrive, therefore, expedients for expoſing the germ, which is undoubtedly contained in the veſicles of the ovarium of large viviparous animals before the acceſs of the male, is the firſt requiſite. Its tranſparency probably contributes as much as its minuteneſs, to render the germ inviſible before fecundation. Whatever would tend to leſſen its tranſparency, and to coagulate the embryo, would reduce it within the power of glaſſes; ſuch experiments, ſo likely to illuſtrate the important and obſcure ſubject of generation, have been little practiſed; and I foreſee, that if you will but conſent to deſcend into this of mine, you will bring away unexpected wealth; for Nature treats you as a favourite child, becauſe you underſtand how to interrogate her properly. But though we ſhould not be able to obtain a diſtinct view of the germ in large quadrupeds and birds before fecundation, we may very juſtly preſume, from what we moſt certainly know concerning this intereſting ſubject, that the germ exiſts before impregnation, or that its formation is not owing to the concourſe of the male and female, but bears date *à primordio*. Haller's ler's demonſtration of the pre-exiſtence of the chicken is not ſtrictly direct; it does not produce the germ itſelf before fecundation, but eſtabliſhes only the continuity of its membranes with the yolk, which inconteſtibly exiſts before fecundation, This inquiry has, I think, been abandoned too ſoon; it has too haſtily been determined to be beyond the reach of human abilities. I am perſuaded, that if it had been conducted with the ſame perſeverance, pains, and ſagacity, as you have ſhewn in your profound reearches concerning the animalcules of infuſions and amphibious animals, far more direct proofs of the pre-exiſtence of the germ in the females of oviparous and large viviparous animals, would have been obtained. There is ſome contrivance, not yet conceived by phyſiologiſts, which, the moment they ſhall diſcover it, and put it in practice, will afford the demonſtration we ſo much deſire. Men ſhould never be in haſte to conclude that any inquiry is impracticable, merely becauſe no relation appears between the preſent means, and the fact to be aſcertained; for have all the combinations of theſe means been tried? If not, who can be certain, that he is acquainted with all the relations between theſe means and the fact, which is preſumed to exiſt, and propoſed to be aſcertained? This reflection is ſtrengthened by numberleſs inſtances, recorded in every page of the hiſtory of natural philoſophy, and more eſpecially of phyſiology. Conſider how many inventions, for which the ancients would ſcarce have dared to hope, even if they had had a diſtant glimpſe of them, have enriched theſe

these sciences! Could they have imagined, that the artificial fecundation of the germs of various animals would one day be accomplished, and that chickens would be hatched without the aid of animal heat or of a stove? Need I mention those admirable chirurgical operations, the miracles of the healing art, which are scarce credible even to those who behold them? We are as yet incompetent judges of what is impossible in philosophy; for we always decide from our present acquisitions of knowledge, which bear no proportion to Nature. Nature is immense; the possible combinations of beings have no bounds, and the human understanding is almost always too hasty in its decisions. The conviction of our ignorance or moderate attainments, ought to preserve us from despairing of any thing in physics; we should say to ourselves, what I cannot discover, another will lay open to every eye. But enough upon a subject capable of affording a small volume of practical reflections; it is time to come to the interesting sketch of the work you are about to publish. I shall be guided by the numbers of your paragraphs.

XIII. I find here, that you have imagined a curious contrivance to prevent the fecundation of the green aquatic frog. You do not particularize the experiment, and it excites my curiosity. I must therefore take advantage of your friendly offer, to answer the principal questions suggested to me by the perusal of your sketch. You may be sure, that I shall make use of your answers. You shall be my oracle, and I am certain that its replies will be more intelligible and instruc-

tive

tive than thoſe of the Delphian oracle. How have you contrived to prevent fecundation in this frog? Probably the experiment has ſuggeſted new information concerning the myſtery of fecundation; for nothing here ſtands unconnected.

XXVI. I cannot here underſtand the ſenſe of your expreſſions, " the fecundation of the eggs, though effected out of the body, penetrates however a little way into it." Is the action of the ſeed propagated by the gelatinous matter enveloping the eggs? But I ought not to try to find your meaning by conjecture.

LIX. I am glad that you have diſtinctly ſeen the circulation of the blood in tadpoles, before they yet ſhewed any ſigns of motion. Many other inteſtine movements doubtleſs take place in germs, before they are ſufficiently developed to move their little limbs. If germs are all originally encloſed one within another, many inteſtine motions muſt have happened in them ſince the creation. But this admirable ſpectacle is reſerved for thoſe ſuperior intelligences, whoſe piercing view penetrates into the moſt hidden ſprings of the machine of this world. Much has been ſaid of the involution (*emboîtement*) of germs; the term is improper: germs are not little boxes encloſed one within another; they muſt have been integrant parts of the firſt organized bodies that came from the hand of the Creator. I have inſiſted on this point in one of my new notes on the *Contemplation*. It is of conſequence to fix the meaning of terms preciſely.

LXXV, LXXVI. You are then convinced, that the eggs of ſcaly fiſhes loſe by deſiccation

tion the power of being productive. You have ſeen the ſame thing happen to the ſuppoſed eggs of tadpoles; and you conclude, that the aſſigned explanation of the re-peopling of dried ponds is falſe. I was therefore miſtaken, when in *Art.* CCCXVIII. *of the Conſiderations on organized bodies*, I thought of applying to the eggs of fiſhes what my illuſtrious friend Trembley had obſerved with reſpect to thoſe of the *tufted polype*, which may be kept ſeveral months in the dry without loſing the power of producing. I however threw out an invitation to Naturaliſts on the ſubject of fiſhes eggs: I ſaid, "that it would be a curious experiment to keep different fiſhes eggs for ſome time in the dry, and then diſtribute them in proper places. This ſimple trial would aſcertain, whether in ſuch a ſtate they can ſerve to perpetuate the ſpecies. Nature is not ſubjected to undeviating exactneſs: In her operations there is a latitude which the Naturaliſt ought to ſtudy, and which experiment alone can detect." You have accepted my invitation, and your experiments have not been favourable to my conjecture. As you have not pointed out your manner of proceeding, I am unable to judge whether it excludes the moſt common natural circumſtances in ponds and pools. Many trivial particulars may produce a variation in the reſults. You eaſily gueſs what I have in view; but I am not leſs inclined to acquieſce in the falſehood of my conjecture.

LXXX. *Strange way in which the male newt impregnates the female without copulation.* I have never ſeen the fecundation of the newt; but my curioſity is much raiſed: I beg

I beg you will inform me in what this ſtrangeneſs conſiſts! My brain has long been diſpoſed, by frequently taking cognizance of prodigies, for the admiſſion of the ſtrangeſt things; for ſuch ſurely is the reproduction of the limbs in the newt, which has engaged my attention for ſome years, and ſtill engages it. I ſhould probably have ſeen the act of fecundation, if I had kept individuals of different ſexes in the ſame veſſel; but my only aim was to obſerve with my own eyes theſe beautiful organic reproductions.

LXXXIV, LXXXV. You treat in theſe articles of the natural fecundation of the eggs of the newt. I am totally unacquainted with it, and ſhould be obliged by a brief explanation.

XCVI. *What foreſight do amphibious animals ſhew in the multiplication of the ſpecies?* Nothing in the ſtudy of animals intereſts my curioſity ſo much as their *foreſight*. We frequently commit miſtakes on this ſubject, and are apt to form very unphiloſophical ideas; but of this I have ſaid enough in the *Contemplation*, and elſewhere. We impute to animals, with wonderful facility, our own foreſight and our reaſoning faculty. Could they judge concerning us, they would reduce us to their ſtandard; for they would attribute to us their ſenſations and actions. Authors have run into the moſt oppoſite extremes in conſidering this topic; I have ſought the medium, and hope that I have found it. You have a good right to decide.

XCVII, XCVIII, XCIX. I find here ſomething relative to the induſtry of animals, and I dwell upon it with pleaſure, having devoted one whole chapter of the *Con-*

templation to the amours of the toad which you conſider here. You enquire, *Why the male frog and toad embrace the female ſo cloſely and ſo long?* I had preſumed, that ſuch a long continuance had ſome ſecret final cauſe; but I was unacquainted with any obſervation which could guide me to that cauſe. I expect from your friendſhip the inſtruction which I want. I ſhould ſuſpect, that the embraces tend to facilitate the deſcent of the eggs or the tadpoles into the ducts, and thence into the uterus, and moreover, perhaps to aid the expulſion of the embryos. I meet in every part of your ſketch with enigmas, and hazard my conjecture without hoping that it is the true explanation.

CII. Has the intereſting obſervation of Mr. Demours, concerning the amours of the toad, of which I made uſe, given occaſion to any critical reflection? Was the obſerver deceived? He certainly ſtood in a good ſituation for obſerving.

CV, CVI. Here you do not ſeem inclined to abide by the current accounts of the fecundation of ſcaly fiſhes. You ſpeak of *uncertainties*, what are they? Do you doubt whether the male emits his *milt* upon the eggs? Or whether he repeatedly rubs the belly of the female? Or of both theſe accounts? My own faith was very unſettled upon theſe points. I could no where find deciſive obſervations. It is aſtoniſhing ſometimes to ſee Naturaliſts remain ſo long and ſo quietly in doubt, about the moſt intereſting queſtions, and make no attempt to ſolve them by the eaſieſt obſervations and experiments. The mind, naturally ſo active, is yet often very indolent. To contrive an inſignificant experiment, or a ſmall apparatus, requires as great an effort as to undertake a journey. Such obſervers only as you are always alert; ſuch only have their minds always in action and forming combinations. You have made more diſcoveries in five or ſix years, than whole Academies in half a century. I, however, who know the labourer, and the richneſs of the harveſt, am not at all ſurprized. When, in 1765, you obligingly aſked my opinion on the department of Natural Hiſtory to which you ſhould apply, I eaſily foreſaw how much that ſcience would one day be indebted to you. Your firſt production on the animalcules of infuſions, fulfilled my prediction, and your intereſting letters upon the wonderful reproductions of the earth-worm, the ſnail, and the newt, were new confirmations of it. I have contemplated, from the ſilence of my retreat, your gigantic progreſs in that fine career.

CVIII. From this article I learn, that the fecundation of the newt differs from that of other animals; but I am not informed in what the difference conſiſts. The modifications of the great laws of Nature chiefly excite the attention of the philoſophical Naturaliſt. They ſtrike him with the greater force, as they ſhew the endleſs variety of the CREATOR's reſources, and of thoſe ſubordinate means, by which his WISDOM prepares the firſt developement of animated beings. It was not conſiſtent with the œconomy of the world, that all generations ſhould exiſt together in a ſtate of complete evolution. The earth could neither have con-

contained them, nor ſupplied them with food. They were, therefore, placed one within another, in a ſeries continually decreaſing, and loſt at laſt in the abyſs of infinite littleneſs. The generations are therefore developed, one by means of another, and their growth is proportional to the order of degradations. Thus they are gradually advancing out of impenetrable darkneſs, and at length arrive at the term, which ſeparates the viſible from the inviſible, and from which, by the aid of fecundation, they inſenſibly ariſe to the degree of perfection competent to the ſpecies. But as animated beings are greatly diverſified, ſo likewiſe are the laws which preſide over their developement. Hence reſult many varieties in the forms which they ſucceſſively aſſume, and in the mode and effects of fecundation. The delineation of theſe phaſes and varieties, conſtitutes the moſt engaging part of the hiſtory of generation.

CXXIII, CXXIV. From your numerous experiments on artificial fecundation it appears, that attempts of this kind upon germs, while yet in the ovarium, or at the upper end of the ducts, will fail. I can, I think, aſſign the reaſon. The ſeed acts on theſe fetuſes as a ſimple ſtimulant. Now there is an original relation between the latent power, which cauſes the irritability or the contraction of the muſcular fibre, and the preſent ſtate of that fibre. If it has not yet attained the neceſſary degree of conſiſtence, it will not be in proportion to the mode of action of that power, and its impreſſion will therefore produce no effect. The germ muſt have arrived at a determinate growth before it can be ſuſceptible of irritation. Such was the reaſoning of the great Haller.

CXXVIII, CXXIX. I find here a particular which embarraſſes me. *Artificial fecundation*, you ſay, *does not ſucceed in the newt with pure ſeed, and it is neceſſary to dilute it with water*. But *pure ſeed* is the efficient cauſe of natural fecundation: how then comes it to fail in the artificial? does the air inſpiſſate it too much, or is it mixed with ſome thinner fluid at the inſtant of its emiſſion from the male. I know not what to think. You will inſtruct me.

CXXXIV. *The ſeed does not loſe its prolific virtue, though incorporated with other liquors.* I have great pleaſure, my dear philoſopher, in ſeeing you ſound Nature, by means of combinations which ſhe never made. Preceding phyſiologiſts did not conceive the idea of ſuch combinations. But what liquors did you mix with the ſemen? it does not, it ſeems, mix intimately with them; there is no chemical ſolution, ſince the ſperm retains its prolific virtue—it is at leaſt very certain, that it is not decompoſed by theſe different liquors, for decompoſition would deſtroy its ſtimulating power. How do you reaſon on theſe facts?

CXLIII. *Three grains of ſeed incorporated with eighteen ounces of water, retain perfectly their fecundating virtue in the green aquatic and in the tree-frog. Three grains diluted with two hundred and ſixty-four ounces of water, do not altogether loſe their efficacy.* This would never have been ſuſpected; what wonderful energy muſt this ſingular ſtimulant poſſeſs!

possess! since it is not sensibly weakened, when three grains are mixed with eighteen ounces of water, and is even perceptible, when they are diluted in two hundred and sixty-four ounces! these three grains of seed apparently are diffused through this body of water, as three grains of musk are diffused through the air of a large apartment; they nevertheless act upon the smell, and irritate the nervous fibres; this irritation illustrates the effect produced by seed diluted in a large quantity of water, upon the muscular fibres of the heart of the embryo.

CXLIV, CXLV. *Other remarkable experiments upon a little seed, mixed with a great quantity of water.* Experiments so new and instructive, could not be too much varied; and it is with great satisfaction I observe, that you have taken advantage of Nature's complaisance. I beg you will give me further instruction on this head.

CLII. *The seed fecundates tadpoles by penetrating into their diminutive bodies. Small openings for its admission.* Such circumstances have always been more interesting to me, than any thing in the history of fecundation. I have, always, you know, presumed that fecundation was effected *from without*; and you may recollect, that I once desired you to search for the little pores, which I imagined Nature had wrought in the covers of the germ, for the introduction of the seed: You have now discovered them, and I congratulate you most sincerely. The discovery is of high importance, and I am quite impatient for the particulars. I have observed in a new note, Chap. X. part. 7. of the

Contemplation, "What happens openly during the fecundation of the eggs of amphibious animals, takes places in other claſſes in the obſcurity of the ovarium. The egg is always fecundated externally, both in oviparous and viviparous animals. The ſuppoſition followed very naturally, after the embryo was allowed to exiſt entire in the egg: the obvious inference was, that the ſeed acted only as a nutritious and ſtimulating principle. But this ſimple and philoſophical notion of fecundation could not be conceived by thoſe who reject all organic preformation, and imagine, that the embryo is mechanically formed by certain powers of affinity *(forces de rapport)* or by the ſucceſſive juxta-poſition of certain *molecules* iſſuing from the male and female, which had been moulded within them.

CLVIII. *Whether the ſmall portion of ſeed, employed by Nature for the impregnation of amphibious animals, affords reaſon to ſuppoſe that large animals alſo are impregnated by ſo inconſiderable a quantity.* I am very deſirous of knowing your thoughts on a ſubject, which has never been properly diſcuſſed, becauſe, before it could be properly diſcuſſed, it was neceſſary to make thoſe fine experiments, which you have ſo well executed on amphibious animals, and which yet were not ſuppoſed to be poſſible. If the ovaria of an ewe, bitch, or cow, could be laid bare without endangering life, your queſtion might be decided by experiments like thoſe on amphibious animals, which ſuggeſted to you ſo many new truths. You might touch with the end of a pencil, dipped in the ſeed of the male,

male, one or more of the *veſiculæ* of the ovarium; and if the wound could be healed without deſtroying the effect of fecundation, you would find the quantity of ſeed neceſſary for this purpoſe in large animals. This ſingular experiment would perhaps ſucceed better on the greater oviparous ſpecies. If it is to ſucceed at all, it muſt be in your hands. You have accuſtomed me to expect much from your dexterity and perſeverance. Should you only be fortunate enough to perceive an incipient evolution of the germ, it would be ſufficient for our inſtruction. There are obſervations which directly prove, that the fetus may be developed in the ovarium. You might alſo try to inject different quantities into the matrix. Should the ſmalleſt portion prove as efficacious as the greateſt, it would become very probable, that the fecundation of large animals differs but little in this reſpect from that of the amphibious claſs.

CLIX. *The ſeed of amphibious animals ſeems to be a ſimple ſtimulant, and not a nutritious fluid.* I think I perceive the foundation of your concluſion. You cannot reconcile the ſmall quantity of ſeed ſufficient for fecundation, with the idea of a fluid, deſtined to ſupply all the parts of the embryo with their firſt nutriment. You calculate (CLV), that the bulk of this portion of ſeed, is to that of the embryo, as 1 to 1064777777. There can, therefore, be no ſort of proportion between the nutritious fluid, and the embryo to be nouriſhed. I ſhall not conteſt the juſtneſs of your inference, as far as it regards amphibious animals; but I do not think you can extend it to large animals. The mules produced

produced by them furniſh facts, which render it impoſſible to doubt, that the ſeed modifies various parts of the embryo: I have given ſeveral inſtances; the ſeed is therefore carried to theſe parts, and how can it be imagined to change their forms and proportions without penetrating into them (*a*)? Conſider, in particular, the larynx of the *great mule*. I am brief; but you, who underſtand my principles as well as I do myſelf, will eaſily comprehend my meaning.

CLXVII. *The ſeed is not a ſpirituous nor very volatile liquor, as ſome philoſophers have imagined.* I am glad you have aſcertained this; but after having ſhewn what the ſeed is not, have you diſcovered what it is? We well know, that it is one of the moſt highly wrought fluids in the animal body. Have you made any chemical experiments upon it? I have always entertained a ſuſpicion, that it very much reſembles the nervous fluid. I have

(*a*) When the experiments related in this work, ſhall be attempted upon large animals, as there is every reaſon to hope that they ſoon will, with ſucceſs, I do not think there is any great temerity in predicting, that this favourite hypotheſis of Mr. Bonnet will be proved to be falſe. It was long ſince rejected by Haller, the moſt candid and acute examiner of phyſiological hypotheſes; and Spallanzani has furniſhed a very powerful additional argument, derived indeed from analogy, but more cogent than any proof in favour of it. There are numberleſs examples, of parts of the ſyſtem being deeply affected by ſubſtances not in contact with them. Nervous ſympathy is called in to explain ſuch phænomena, and to what elſe is it poſſible to impute them? Phyſiologiſts will, in like manner, I ſuppoſe, deduce the changes produced by the ſeed of one ſpecies upon the embryo of another, from the ſame cauſe. T.

have proved, that irritability muſt depend on a very active fluid, diſſeminated among the muſcular fibres. *Contemp. Part. X. Ch.* 33. The ſeed of amphibious animals, which is the ſtimulant of the heart of the embryo, may have a certain affinity (*rapport*) with the fluid diſperſed in the fibres of this impelling organ. Here we find depths that cannot yet be fathomed.

CLXVIII. *The electric fluid accelerates the evolution of fecundated tadpoles, but does not animate thoſe which are impregnated.* I thank you for having tried, as I propoſed, whether the electric fluid might be ſubſtituted inſtead of the ſeminal liquor, in the artificial fecundation of tadpoles. It did not anſwer, and you may imagine, that I do not wonder at the failure. It was proper to make the trial. *Que ſait-on* is a queſtion that frequently recurs in natural philoſophy and natural hiſtory. You have at leaſt found, that the electrical fluid forwards the evolution of fecundated tadpoles, a new obſervation, to be added to thoſe recorded in the hiſtory of electricity.

CLXIX, CLXX. *Various other liquors are incapable of effecting artificial impregnation.* That talent of combination which you poſſeſs, and which charactizes the true naturaliſt, gave me reaſon to expect theſe various experiments. Nature, by her negative reſponſes, has afforded you no leſs information, than when ſhe has anſwered in the affirmative.

CLXXI. *The ſeed of the newt is incapable of fecundating the embryos of frogs and toads, and reciprocally. This is alſo true of the ſeed of toads and frogs reſpecting one another.* Thus

though

though Nature produces mules between large animals, and even between insects and plants, she refuses them to the amphibious tribe. You very properly questioned her on a subject so important; from her answers we learn, that she has not here allowed herself any latitude. This is very remarkable; for at first sight, the toad appears to differ much less from the frog, than the horse from the ass. If the contrary had happened, and you had obtained *mules* by the artificial impregnation of amphibious animals, you would not have had such good reason for concluding, that the seed of these animals acts but as a stimulant. It would remain to inquire, why Nature has here set herself such narrow limits. If it should be said, that in species apparently so nearly allied, one degree more of approximation would have destroyed the specific character, this would be to assign the final, but not the efficient cause.

CLXXII. *The eggs of the winged insect produced from the silk-worm, artificially fecundated by the author.* An Italian, the famous Malphigi, first imagined this experiment: another Italian, the second Malphigi, first succeeded in it. Above eighteen years ago, I exhorted naturalists to repeat this interesting experiment; and in this long interval, the philosopher of Reggio alone has undertaken it. What satisfaction must he receive on this account! But he will not confine himself to the eggs of this species; he will proceed to those of other butterflies. He will try to fecundate the eggs of one species with the seed of another: he will desire to know, whether butterflies and moths resemble amphibious

phibious animals in this reſpect; and he will not, I am ſure, neglect making the ſame trials upon other different inſects. Reaſoning will not illuſtrate this ſubject: experiment alone can ſupply the information we deſire.

From all your experiments, you deduce a concluſion highly pleaſing to me; all the dreams of modern epigeneſiſts are diſowned by Nature. You know how averſe I have been all my life to this ſyſtem; I have combated it in all my writings, and when my late illuſtrious friend HALLER was inclining to this hypotheſis, I had the courage to reſiſt him, notwithſtanding the impreſſion his authority made on me. The chicken came to my aſſiſtance, and the great phyſiologiſt declared againſt epigeneſis.

I have gone through the principal articles of your table: many things are left behind, but the reviſal of my works does not permit me to attend to you as much as I could wiſh. I paſs to your kind letter of the thirteenth of March, which I have not yet anſwered.

I knew not that the celebrated Merian had obſerved the Pipa before Ruyſch. I have made uſe of the paſſage from that heroine, which you obligingly tranſcribed for me (*a*).

(*a*) In a letter, written March 13, 1780, to Mr. Bonnet, about his Memoir on the Pipa, inſerted in Rozier's Journal for 1779, and of which I ſpeak incidentally in Sect. CIV, I informed him that Merian, and not Ruyſch, as he ſuppoſed, firſt ſpoke of this ſingular animal; and I moreover communicated to him the erroneous opinions of Vallifneri, who affirmed, that the kind of uterus containing the young toads, is upon the back of the male, and not of the female. To this paragraph of my letter Mr. Bonnet alludes.

I have inſerted it in an addition to my Memoir. I have made the ſame uſe of that from Valliſneri, which was alſo quite unknown to me. Your cenſure of his negligence, in not aſcertaining the ſex of his two pipas, is juſt. I have diſſected, or rather flead the pipa left in our bottle; but the fatigue my eyes underwent, together with the quantity of ſpirits which had penetrated into the inſide, hindered me from obſerving diſtinctly every thing that lay before me. I made my inciſion on the belly, and after turning aſide the ſkin, perceived the muſcles and internal teguments. Having removed them, I diſcovered the inteſtines. I was firſt ſtruck by a number of bright yellow grains that were diſperſed over the viſcera. In order to learn whether their ſhape was regular, I obſerved them with a glaſs, but it appeared to vary a good deal; ſome being round, others oblong, and others again like quadrilateral plates. The ſtomach was oval, muſcular, and thick; it was filled with many ſmall brown fragments, very brittle, ſome a line or two in length, and pretty much reſembling fragments of the leaves of plants.—But to come to our chief object, I could not perceive any veſtige of an ovarium; I ſought for the teſticles, and found, near the bladder, an oblong fleſhy body, which might perhaps be a teſticle, but I could not find that on the other ſide. I could not, indeed, perform this diſſection according to my wiſhes; I ſaw juſt enough to form an idea of the great apparatus of viſcera, which this ſingular toad offers to the attention of the obſerver; every one of which might employ him whole months, if he he had the inclination or the ability to ſtudy it as Lyonet ſtudied his eruca. To refute Valliſneri's opinion, it is not abſolutely neceſſary to have recourſe to diſſection. Mr. Fermin's obſervation, of which I have given the ſubſtance, Sect. CCCXXVII, *Corps organ. n.* 1. leaves no room to doubt that the female's back is furniſhed with the cells, ſince he diſſected her, ſaw the ovarium, and was an eye-witneſs of the union of the ſexes. I am glad my Memoir afforded you ſatisfaction. I wiſh the queſtions at the concluſion may induce ſome Naturaliſt to examine more particularly an animal ſo remarkable, and as yet without parallel.

I thank you for the Italian extract from your reſearches on the movement of the blood, which I could not read in your excellent work. Mr. Saladin tranſlated it, and I ſhall borrow from it ſome curious particulars for a note on Part X. of the *Contemplation*. I aſk nothing further on this ſubject, for my editors preſs me to finiſh as ſoon as poſſible. The ſubſcribers grow impatient, and threaten to withdraw their contributions. I have been obliged to write a ſhort letter in excuſe of my involuntary delay, and to ſatisfy them about the delivery of the ſubſequent parts.

Your concluſions, relative to the blood, have entertained me highly. You firſt have diſcovered an important truth; the impulſe given by the heart is perceptible to the commencement of the veins. That the motion of the blood does not, as phyſiologiſts ſuppoſed, become ſlower at the extremities of the arteries, is another new obſervation of equal importance. They were miſtaken about the power

power or powers of circulation. They aſſigned to the heart auxiliary forces; and you have ſhewn, that the heart is the ſole impelling power. You have alſo proved, by an ingenious contrivance, that the changes of the colour of the blood, from yellow to reddiſh, and then to red, are mere illuſive appearances. Haller warmly conteſted this point, but your mode of proceeding, I think, carries conviction. The vaſcular membrane, ſeparated from the yolk of the egg, and obſerved upon a plate of glaſs, exhibits red veſſels on the firſt days of incubation. You almoſt every where deſtroy error, and eſtabliſh truth. I moſt ſincerely bid you farewell. You ſay nothing of the fecundation of plants: I have not been able to repeat your experiments this year.

END of LETTER I.

LET-

LETTER II.

Genthod, January 13, 1781.

I PERCEIVE by your intereſting reply, my dear and celebrated friend, that our opinions on ſeveral points coincide; this coincidence affords me the greater pleaſure, becauſe it ſhews, that I have reaſoned juſtly on ſeveral of your experiments. But ſuch a coincidence is no new thing between you and me, for how often have we *converged* in like manner on ſeveral topics of Natural Hiſtory? It may be ſaid, that my ſoul ſometimes paſſes into your brain, and your's into mine. I owe you many acknowledgments for having interrupted the compoſition of your work on the generation of plants, in order to write that long and excellent letter, which you call upon me to anſwer. I am ſurpriſed that you have been able to do it in two days. I am not ſo happy as you in this reſpect, being able to allot a few hours only in a day to compoſition; ſo that when I write letters of eighteen or twenty pages, you may be ſure that they have taken up at leaſt twelve days. I muſt therefore now, in my turn, ſuſpend my own labours, for the ſake of anſwering the principal paſſages of your letter of the 12th of December. I ſhall follow the order of your numbers, or rather of my own in my laſt letter, which you yourſelf followed, and to which you replied.

I. I doubted not but the experiments which I propoſed to you, in order to detect the germ in the ovarium before fecundation, would likewiſe ſuggeſt themſelves to your conſideration (*a*). You ſeem not to expect much

(*a*) In order to elucidate this paragraph, I ſhall tranſcribe the paſſage of my letter to which it refers. Mr. Bonnet, in his firſt letter, obſerved, that ſearching for the embryo in the ovaria of quadrupeds and birds, before the acceſs of the male, would certainly illuſtrate Natural Hiſtory: He was of opinion, that I might make ſome advances in this dark enquiry. I reply, " That the idea of turning my attention towards the ovaria of quadrupeds and birds, had often ariſen in my mind; but that I had never enjoyed ſufficient leiſure to put it in practice. The urgency with which you recommend thoſe reſearches, is a powerful incentive to me to attempt them, and perhaps I may do this ere long; but I confeſs that my expectations are not very ſanguine. I fear, leſt the extreme minuteneſs and tranſparency of the germ ſhould prevent me from diſcerning it. It is, however, better to undertake unpromiſing experiments on a ſubject ſo intereſting, than to leave them unattempted.

" Having lately had occaſion to read ſome of Reaumur's memoirs on inſects, I met with an obſervation of an Italian Naturatiſt, very analogous to mine, upon the pre-exiſtence of the fetuſes of the frog and toad. It is in Vol. III. P. 1. Mem. 7. Reaumur, ſpeaking of the grub of the little beetle (*ſcarabæus*) of the lily, after the fine diſſertation of Patarol, ſays, " Mr. Patarol thinks, that the grubs cannot be ſaid to come from the eggs, but that every egg appears to be changed into a grub. The reaſon that induced him to propoſe ſo ſingular a notion, was, becauſe he could not find, with whatever care he examined, any empty ſhell." Obſerve, I beg of you, the concluſion of the French Pliny; he appears deſirous of repeating the obſervation, though other occupations prevented him, it would ſeem, from executing his intention. I am reſolved to repeat it next ſpring, and I think Reaumur did not ſet a ſufficient value upon it."

from

from them: you preſume that the extreme minuteneſs, as well as the tranſparency of the germ, would conceal it from all your reſearches. You do not however deſpair; and you give me room to ſuppoſe, that my exhortations contribute to encourage you: but the great ſucceſs which has crowned your reſearches on other occaſions, might ſerve to perſuade you much more powerfully, that you will not labour on the preſent in vain. A lucky accident, ſome unexpected and unforeſeen circumſtance, may afford you the greateſt aſſiſtance. You will create ſuch fortunate occurrences as do not happen in the ordinary courſe of Nature, and as will ſurprize you with a full view of that which Nature concealed from Malpighi and Haller. It ſeems to me, that the firſt ſtep neceſſary is to find the means of diminiſhing the tranſparency of the germ without altering its ſtructure; for in my opinion this, rather than its extreme minuteneſs, keeps it concealed from the moſt piercing looks of the obſerver. A drop of vinegar, or ſpirit of nitre, poured on the cicatricula of the egg, by a little condenſing the moiſture which dilutes the ſolids of the germ, may perhaps render them perceptible. You might alſo try other liquors. Two other means ſuggeſt themſelves to my mind: The firſt conſiſts in endeavouring to ſpread a liquor coloured by ſome vegetable tincture over the yolk: how do we know but the veſſels of the yolk would abſorb this tincture, and carry it to the germ? Should it only colour the contiguous parts, it would at leaſt ſhew its place or point. The action of the veſſels ſhould be aided by a gentle heat. The ingenious pro-

 ceſs

ceſs employed by Mr. Beguelin, to ſhew the Prince Royal of Pruſſia, the ſucceſſive progreſs of the chicken in the egg, would not be unſerviceable to you in your attempts. Again, who knows but a certain degree of heat would contribute to render the germ apparent, by coagulating its lymph? To ſubſtitute the ſemen of the cock, or any other bird, would be another means of attaining the ſame end. As the ſemen is, beyond controverſy, the liquor which has the greateſt influence upon the germ, it ſeems to be beſt adapted to produce ſome ſudden change, ſuch as might render it acceſſible to the microſcope.

It would be, at the ſame time, an attempt to produce the artificial impregnation of the germ in the egg. Mr. Beguelin's apparatus would be very convenient for this purpoſe. Should a drop of ſeed render the embryo immediately perceptible, it could not be objected that the ſeed had conveyed it into the *cicatricula*, as it has been ſuppoſed that the *farina fecundans* does the plant into the grain; for Haller's obſervation, as well as your diſcovery of the exiſtence of the tadpole before fecundation, would deſtroy the objection. Laſtly, it would be proper to try to obſerve the *cicatricula* of unimpregnated eggs in the ſolar microſcope, which enlarges objects which are in ſome meaſure tranſparent more than any other. Need I add, that you will not be ſeeking for ſomething which does not exiſt, ſince you have the ſtrongeſt reaſons to ſuppoſe, that the germ does really exiſt in the *cicatricula*.

Curious experiments have ſhewn, that the perſpiration of the egg, notwithſtanding its cruftaceous cover, is confiderable. Various internal changes proceed from this. You will therefore obferve the *cicatricula* in unimpregnated eggs kept for a longer or fhorter time. Perhaps drying the *cicatricula* a little upon a plate of glafs, before it is obferved by the microfcope, may be of ufe. The approximation of the elements of the folids, may fomewhat leffen the tranfparency of the germ.

It will be proper, my dear Malpighi, to obferve the real or fuppofed eggs of the beetle of the lily. The paffage you have quoted from my illuftrious mafter fhews clearly, that he faw the neceffity of repeating Patarol's obfervation. It is to be regretted that he did not do it, or at leaft inform us that he had; but we may reafonably doubt, whether this illuftrious Naturalift imputed to this obfervation the fame degree of importance as you and I. He had not meditated fo much upon generation, and particularly upon the interefting queftion of the pre-exiftence of the germ. You may eafily fuppofe, that I fhall not be much furprifed if you fhould one day inform me, that the fuppofed eggs of this beautiful little infect are not really eggs, but the grub difguifed under the appearance of an egg. If fo, thefe imaginary eggs would agree with thofe of frogs, and we fhould have a new proof of the pre-exiftence in queftion. You are acquainted with the eggs of the queen bee; I have fome fufpicion, that they are not real eggs, but the maggot itfelf, in a fhape little different from that which it affumes after fecundation. But the vivaporous flies, of which Reaumur has written the curious

 hiftory,

hiſtory in Vol. IV. will ſurely afford new proofs of the pre-exiſtence of the embryo in the female. The ſpiral matrix, deſcribed by the author, is a beautiful organ; the different convolutions he informs us, conſiſt of more than 20,000 maggots, arranged lengthways by the ſide of each other.

XIII. I am obliged to Meſſrs. de Reaumur and Nollet, for thoſe little breeches of waxed taffety, which they contrived for the male of a certain ſpecies of the frog, in order to diſcover the manner in which he impregnates the female, and I am not leſs obliged to you for the repetition of this ingenious experiment. The male, which you cloathed with ſuch breeches, did not accompliſh the impregnation of the female, becauſe the ſemen was intercepted. Since this liquor impregnated artificially the tadpoles to which you applied it, there can be no doubt but that it was real ſemen.

XXVI. You are then of opinion, that the ſuſpicion I intimated in this article is not without foundation. I learn from this paragraph of your letter a new truth, "that in the TREE-FROG, the tad-poles are ſometimes found fecundated, though they remain as yet in the rectum; whether this happens in conſequence of the ſemen inſinuating itſelf into the orifice of that gut, or becauſe the tadpoles, ſcarce out of the rectum, and already moiſtened by the ſemen, are perhaps drawn into it by the motions of the female at the inſtant ſhe is ſurprized by the obſerver." Both theſe explanations appear much more probable than mine.

LXXV, LXXVI. I am always a great gainer, when, according to your wiſhes, I point out to you new experiments to make. You have then made upon the fecundated eggs of fiſhes that which I indicated, (Art. 418. Corps organ.): in order to aſcertain, whether theſe eggs might be kept in the dry, like thoſe of the tufted polypus; and you have found, that they do not poſſeſs this privilege. Your various ways of proceeding, permit me not to doubt of the truth of the reſult. You have carried this experiment ſtill farther to the fecundated embryos of frogs and toads, and you have found, that they do not, any more than the eggs of fiſhes, poſſeſs the property of keeping in the dry. My hypotheſis then, with reſpect to the re-peopling of dried ponds, is inſupportable: but may not this privilege, which hath been refuſed to the eggs of fiſhes, have been accorded to fiſhes themſelves in the ſtate of infancy, or at ſome other period of their life. I am very deſirous of knowing the conjecture you ſubſtitute inſtead of mine, and which you intend to explain in your work. Reading this paſſage over again, I perceive a circumſtance that had eſcaped me; you ſay, "that you left your fiſhes eggs to dry in the ſhade, *ſometimes on the mud*, where you found them, and ſometimes in veſſels." But may not the fecundated eggs of fiſhes require, like the *Rotifer*, to be covered with ſand, in order to be preſerved in the dry? I can ſcarce ſuppoſe, that you neglected covering them with the mud on which they were laid by the female, well knowing, that in your experiments you neglect nothing (*a*).

LXXX. Your account of the ſingular manner in which the male newt impregnates the female has afforded me great pleaſure; the whole was entirely new to me. The newt is then very chaſte in his amours; no true copulation takes place between the two individuals; only a few careſſes on the part of the male, which prepare the female for fecundation. The male darts his ſemen into the water; it forms a little whitiſh cloud, which ſurrounds the open and ſwoln anus of the female, and ſhe is fecundated. What pity, that the poets were unacquainted with the chaſte amours of this animal—their fictions would have derived great advantage from the hiſtory. That of Zephyr and Flora bears a ſtrong analogy to the fecundation of the palm; in the animal kingdom, I know nothing which reſembles it more than the impregnation of the newt. That of marine plants approaches ſtill nearer, the male does not emit a fine powder, but a liquor, which, in like manner, forms a little cloud in the water.

Your citation of Bomare's Dictionary, has induced me to read the article *Newt*, which I had never before conſulted. Mr. Demours, I ſee, ſeems to have made the ſame obſerva-

(*a*) Mr. Bonnet's opinion of me is too favourable; for in truth I was not aware of this precaution. But were it employed, I do not think it very probable, that the eggs of fiſhes, and the fetuſes of frogs and toads could be preſerved. The thing is however poſſible, and the experiment ought to be repeated with this circumſtance, which I propoſe to do when I have an opportunity.

tions

tions as you, respecting the singular mode of impregnation in this animal. But naturalists will rely more upon you, and it required to be verified by an observer of your eminence. Du Fay's remark also concerning the presence of the gills, when newts are young, and their disappearing totally afterwards, drew my attention. I had observed the same thing with surprize, but never mentioned it, for I was desirous of observing it again, but never did. These gills are a great ornament to the young newt. Du Fay was aware of the organization of the epidermis, mentioned in my first Memoir; and if I had been acquainted with the passage of this academician, I would have taken notice of it.

Since the semen of the male is always mixed with water, I see the reason why artificial fecundation does not succeed with *pure semen*. The observer must imitate nature and dilute it with water. I suppose with you, that the very thick seed of the newt requires dilution, in order to effect both natural and artificial fecundation. In like manner, the wisdom of Nature has found the means of diluting the human semen by the lymph, which so many vessels pour into the testicles and the seminal vesicles. Physiologists tell us wonderful things on this subject.

Few spectacles are so engaging to the philosophic observer, as that presented by the amours of animals, and the various means by which the Author of Nature has ordained that they should preserve their species. Should some able physiologist ever undertake to compose a complete history of generation, he would undoubtedly begin by a delineation of of the amours of animals and plants; and if he ſhould be as great painter as the illuſtrious Buffon, he will be able to engage the underſtanding, without giving any alarm to modeſty; he will produce, not a *phyſical Venus* (*a*), but a *phyſical Minerva*. There is room for ſuppoſing, that the different modes of fecundation, obſervable in different animals, are proportional to the degree of ſenſation accorded to each ſpecies, or, what amounts to the ſame thing, to their capacity for enjoyment. What difference in this reſpect between the fiſh or newt, and the ape, the ſtag, or dog; and in the imperial race of man, how is the phyſical part modified by the moral!

It is certainly very remarkable, that amphibious animals, ſuch as toads and the tree-frog, never depoſit their embryos on dry ground, where they muſt infallibly periſh, and that they always take care to depoſit them in water, their natural element. You even give me to underſtand, that they do not lay them in the firſt water they find, that they never make the diſcharge in running waters, which would carry the young away, and not ſupply them with proper food; but that they conſtantly depoſit them in ſtagnant waters, where the little tadpoles are not expoſed to concuſſion, and where they are always ſurrounded by proper food. This kind of inſtinct very nearly imitates foreſight, and attains its end equally well. But ſince we cannot, in this caſe, admit real foreſight, which belongs excluſively to reaſon or intelligence, it remains

(*a*) *Venus Phyſique*, the title of a book written by M. Maupertuis. T.

to be aſcertained, how our amphibious animals are ſo unerringly determined to quit the ground, for the ſake of laying their eggs in dormant waters. The female, I ſhould imagine, preſſed by the deſire of laying, muſt feel a certain internal ſenſation, which renders her abode on dry ground painful, and inſpires her with the deſire of gaining the water; and ſince ſtagnant waters are not ſo cold as running waters, this may, perhaps, be the reaſon why ſhe prefers the former, not for the ſake of her young, of which ſhe cannot have any knowledge, or foreſee the wants: for it is thus that Nature hath, on all occaſions, provided for the neceſſities of young animals; ſhe has found means to connect their wants with thoſe which the parents muſt feel in certain circumſtances. Your memory muſt ſuggeſt ſo many inſtances, as to render it unneceſſary for me to point them out. Beſides I ſee you entirely agree with me, with reſpect to the foreſight and intelligence, attributed ſo gratuitouſly, and ſo unphiloſophically to brutes.

XCVII, XCVIII, XCIX. I knew not, that your illuſtrious compatriot Valliſneri had entertained the ſame idea as myſelf, concerning the effect of the long continued embraces of male frogs and toads. Nor did I recollect that Swammerdam, on the contrary, had ſuppoſed, that ſo far from facilitating the paſſage of the eggs into the tubes, they rather ſerve to hinder it. I ſhould not then have known, which ſide to have taken between theſe two great authorities, if Nature herſelf had not pronounced her deciſion from your lips. You inform me, that the opinion

of

of Swammerdam, that the females are not embraced by the males, until the eggs have already traverſed the tubes, is not generally true; that it holds only in the *tree-frog*, and by no means in the aquatic frog and in toads, but that Vallifnieri is right with reſpect to the green aquatic frog. In this caſe then, no general rule can be eſtabliſhed, as you very properly remark, and we muſt wait till new reſearches have increaſed the number of facts.

CII. Mr. Demours had raiſed our curioſity to a very high pitch, by his account of the addreſs of the male toad in aſſiſting the female in bringing forth. His details were ſo circumſtantial, that the truth of the fact appeared to be unqueſtionable, and I heſitated not to make uſe of it in the *Contemplation*. But it is really very ſingular, that neither you, my worthy friend, nor Mr. Roeſel, ſhould have ſurpriſed any male toad in this intereſting employment. This would appear to weaken the credit due to the recital of the French obſerver, if teſtimonies ſimply negative could impair the moſt poſitive affirmation. Mr. Demours ought, as you obſerve, to have ſo deſcribed his toad, that we might have known to what ſpecies it belonged.

CV, CVI. Your doubts, with reſpect to the manner in which the impregnation of ſcaly fiſh is effected, are well founded, and we have both reaſoned properly upon this ſubject, by eſtimating the authorities on either ſide. We know at leaſt, from the experiment of Mr. Jacobi, that ſimple diſperſion in water is ſufficient for the impregnation of the eggs. Your idea of employing the Chineſe

Chineſe gold fiſhes to clear up the queſtion, to me appears excellent, and I cannot preſs you too warmly to realize it.

You will ſee much more in a day, than all preceding naturaliſts have ever obſerved. How many intereſting queſtions, yet undetermined, might be decided by the moſt ſimple experiments? The mind has always a greater tendency to imagine and reaſon, than to obſerve and make experiments. To what a train of reaſoning had Digeſtion given riſe, before Reaumur and Spallanzani ſet this function in the cleareſt light by their fine experiments.

CXXIII, CXXIV. You adopt then with me the Hallerian doctrine of embryos lodged in the ovarium, or in the upper part of the tubes of our amphibia, which cannot be fecundated artificially. But you aſſign another cauſe of the fact, which I ſuſpected not, but which appears to me, not leſs than to you, to contribute to produce it; ſince the glairy matter is the firſt nutriment of the fecundated embryo, and ſince this matter does not envelop thoſe contained in the ovarium, or the upper part of the tubes, it is evident, that even if the ſemen could impregnate them artificially, they would ſoon periſh for want of nouriſhment. Your experiments on this ſubject leave nothing to be wiſhed, ſince the embryos you have entirely ſtripped of their gluten could never be impregnated, while thoſe which were only partially deprived of it, were almoſt all fecundated. I know not whether naturaliſts before you, knew the true uſe of this matter.

CXXXIV.

CXXXIV. The blood of amphibious animals, their ſaliva, the juice extracted from their liver, lungs, kidneys, their urine and ours, are then the different liquors with which you have conceived the idea of mixing the ſemen. To theſe you have added vinegar, and none of the mixtures have deprived the ſemen of its prolific virtue. You have only obſerved, that when the urine and the vinegar were in too great abundance, fecundation did not take place; I doubted not but that you would think with me, that the ſemen is not decompoſed by theſe mixtures. But they admirably prove the aſtoniſhing energy of this fecundating liquor. They may further ſerve to guide you to diſcover, which of the animal liquors hath the greateſt analogy with the ſemen: for that, which in equal portion ſhould leaſt impair the virtue of the ſemen, might juſtly be preſumed to be moſt analogous to it; and this would not be without its uſe, in inquiries into the conſtituent parts of the ſemen.

CXLIII. It affords me great pleaſure to find, that we have both had recourſe to the ſame compariſon, in order to illuſtrate the prolific power of ſemen incorporated in very ſmall quantity, with a very large maſs of water. Your example, taken from the poiſon of a viper, of which a very little drop often proves fatal to a great animal, is not either leſs appropriated or leſs inſtructive. Hence you have good reaſon for ſaying, that we cannot be ſurpriſed, that a very ſmall portion of ſemen ſhould be ſufficient to animate the heart of the embryo.

The

The very ſudden action of certain poiſons, ſuch as the venom of the viper, afford room for preſuming, that it is chiefly the nervous fluid that is affected. You remember Reaumur's curious experiments with American poiſoned arrows. A bear pricked with one of them died, I think, in half a minute.

CXLIV, CXLV. In this article, you furniſh me with a detail of the manner in which you have proceeded in your artificial fecundations. I entirely approve of it. It is ſurely very ſurpriſing, that an embryo touched with the point of a needle, which has been dipped in a mixture of three drops of ſemen, and eighteen ounces of water, and which takes up a drop, meaſuring 1-50th of a line, ſhould have been developed as perfectly and ſpeedily as other embryos which were immerſed in ſemen. Your reflection on this occaſion is very juſt; ſince ſo ſmall a drop of ſemen, mixed with ſo large a quantity of water, is ſufficient to animate the embryo, it is very natural to infer, that the ſurplus furniſhed by the male does not concur in the operation. But Nature is never ſparing in what concerns the propagation of the ſpecies: ſhe is determined not to miſs her aim, and ſhe would run the riſk of miſſing it by too great œconomy. She, perhaps, alſo has an eye to the pleaſure the male receives from fruition; for emiſſion is without a doubt a pleaſing ſenſation to him, and this kind mother is deſirous that all her children ſhould find enjoyment; otherwiſe too the male would want an incentive.

CLII. You eaſily conceive, my dear philoſpher, all the attention I have paid to this intereſting interefting paffage of your letter. I imagined, that I beheld with you the fmall pores in the cover of the embryo, contrived for the introduction of the femen. Your details on this point fully prove, that you have not fuffered yourfelf to be impofed upon; and that thefe little mouths, of which I had fufpected the exiftence, are certainly to be found: and fince they are difperfed over the whole cover, and this cover is perforated like a fieve, it can no longer be matter of furprife, that fecundation fucceeds equally well, wherever the embryo is touched with the needle, after it has been dipped in the femen. The queftion now is, whether fuch apertures exift in the covers of the embryos of every fpecies; and how probable is this, after all that has been difcovered concerning the myftery of fecundation: I do not then doubt, and I have never doubted, that if the germ of the pullet, of the lamb, of the calf, were as perceptible as the tadpole, you would detect abforbent pores, fimilar or analogous to thofe in the embryo of your amphibia. I would afk, if we have not the ftrongeft proofs that fecundation is effected *from without*, and if it be thus effected, is it not neceffary, that there fhould be little pores prepared for the reception of the fecundating liquor? Thefe abforbent pores and their dependencies contain, without doubt, anatomical peculiarities, which we fhould admire, if we were permitted to defcend to bottom of the abyfs. Each pore is probably the orifice of a veffel communicating with the heart, &c.

CLVIII. I now come to the moft curious and important article of your excellent letter.

ter. I ſuſpected not, I own, that you had already ſucceeded in the artificial impregnation of the female of a large animal, by means of a ſmall ſyringe, a project I mentioned in my laſt letter. This is one of the moſt important and intereſting diſcoveries, that have preſented themſelves to the notice of naturaliſts and philoſophers ſince the creation of the world. Your mode of proceeding, and your ſcrupulous attention to eſtabliſh, in the moſt rigorous manner, the truth of this artificial impregnation, put it beyond all controverſy. I had no occaſion for your ſecond letter of Dec. 15, to be aſſured, that you had neglected nothing to obviate the moſt ſubtle objection. Your bitch was then cloſely penned up for twenty-three days before the operation: on the 13th ſhe began to be in heat; on the 23d you injected the ſemen, and you kept her in cloſe confinement twenty-five days longer; on the ſixty-ſecond after the injection, ſhe brought forth three whelps well conditioned, very lively, and reſembling both the dam and the dog which had ſupplied the fecundating liquor. Nothing can be more exact or better aſcertained; nothing can be finer or more original than this experiment. I congratulate you ſincerely on your ſucceſs, and what adds greatly to it is, that it was obtained with leſs than thirteen grains of ſemen. This experiment agrees perfectly well with thoſe which you have executed on amphibious animals, and we have good reaſon for inferring, that the doſe of ſemen which produces fecundation in large animals, is very inconſiderable. I even preſume, if you could ef-

 fect

fect the fecundation of the embryos of a large animal in the ovarium, by the procefs I pointed out to you, you would obtain the fame refults as the amphibia afforded; and that a drop of femen 1-50th of a line in diameter, would be fufficient to animate the embryo.

You are now in poffeffion of a fure and eafy way of afcertaining what fpecies can procreate together; and the experiments you propofe attempting next fpring, by putting your voluptuous fpaniel in the company of cats and rabbits, promife not fo fair as thofe which you will make, by introducing the femen of this fpaniel into the uterus of a doe-rabbit and a fhe-cat, and on the other hand, by introducing the femen of the male rabbit and cat into the uterus of a bitch. You hold in your hand a precious clue, which will guide you to the moft important and unexpected difcoveries. I know not, whether what you have now difcovered, may not one day be applied in the human fpecies to purpofes we little think of, and of which the confequences will not be trivial. You conceive my meaning: however that may be, I confider the myftery of fecundation as nearly cleared up. What remains principally to be difcovered, is the formation of the mule, and what occafions the different marks of refemblance between children and their parents; and this brings me to your CLIXth article.

CLIX. You do me great honour, my dear affociate, by fufpending your judgment between Haller and me, with refpect to the manner in which the mule is formed. What! did

did not the authority of the great Haller, in your eſtimation, overbalance mine, which is ſo much leſs weighty? I would not have heſitated a ſingle moment to admit with him, that the ſemen acts merely as a ſimple ſtimulant, could I have accounted for the converſion as it were of the horſe into the mule. His hypotheſis, from its greater ſimplicity, is more acceptable to the mind. But is it ſufficient in all caſes? In order to account for the formation of the *great mule*, is it enough to ſay, that the ſemen of the aſs is a more powerful ſtimulant than that of the horſe; and that hence it ſo much elongates the ears of the embryo contained in the ovarium of the mare; for how then comes a part of the embryo's tail to be obliterated? Why is its crupper ſo ſlender? and above all, whence comes the larynx ſo different from that of the horſe, and ſo nearly reſembling that of the aſs? I cannot, I own, conceive that the inſtantaneous action of a drop of ſemen on the heart of the embryo can produce effects ſo great, ſo permanent; and on the other hand, I have againſt me the complication of my own hypotheſis, of which the expoſition required a long ſeries of propoſitions, which make it appear ſtill more complex, and not to be comprehended, but by readers of great attention and much accuſtomed to analytic deductions. Hence many have committed ſtrange miſtakes, with reſpect to my principles and their application.

There is alſo another circumſtance, which ſeems to militate againſt my hypotheſis; this is the very trifling portion of ſemen which is ſufficient for generation; it is not eaſy to comprehend how a drop of ſemen, ſo diſproportionate to the whole body of the embryo, can ſerve for its firſt aliment. But this difficulty preſſes on Haller as much as upon me; for it evidently implies, that one kind of ſemen acts with more force than another on certain parts, and occaſions a more ample evolution; that the ſemen of the aſs, for inſtance, *impels the blood with greater violence into the arteries of the ear*; theſe are his own terms: he admitted, therefore, that the ſemen of the aſs arrives at the arteries of the ears of the embryo of the horſe, how, otherwiſe, could the ſimple action of this ſemen, on the heart of the embryo, propagate its impreſſion to the ears, and cauſe ſo exceſſive an evolution of them? Beſides, how come the ears to be the only part of the head which grow to ſuch a prodigious length, ſince all partake alike in the impulſe of the heart. Further, Haller ſpeaks of the power poſſeſſed by the ſemen, of occaſioning the growth of the beard, and of lengthening the tuſks of the elephant and the wild-boar: he adds, if it has this power of promoting a greater growth in certain parts of the body which prepares it, than in others, it may have the ſame effect on the body of the fetus which it animates. Would not this ſerve to ſhew, that our author tacitly ſuppoſes a diſperſion of the ſemen through the whole body of the embryo? I ſuppoſe the ſame thing; and you have no greater difficulty than myſelf, in conceiving the prodigious diviſion of which a drop of ſemen is ſuſceptible. What we know of the diviſibility of matter ſmooths this difficulty. It is much to be regretted, that our great phyſiologiſt

ſiologiſt confined himſelf to mere generalities on this ſubject, and that he did not apply his hypotheſis to the explanation of the principal peculiarities offered by the mule. "It is true, ſays he, my anſwer does not explain the mode or the mechaniſm by which the ſemen of the male excites the germ of the ear, and cauſes ſo large an evolution of it. But I am not obliged to explain this mechaniſm, provided my facts are well eſtabliſhed. The influence of the ſemen on the growth of the beard and horns is demonſtrated, though the manner may be perhaps for ever unknown. It is ſufficient to ſhew, that there is a certain power in the ſemen of the male, which determines the growth of the fetus, ſo that certain parts come to be more developed: It would not be more juſt to demand an explication of the mechaniſm by which this is brought about, than the reaſon, why the abſorption of the ſemen of the male produces the growth of the beard."

I ſhould have eſcaped much labour, if in imitation of my illuſtrious friend, I had contented myſelf with repeating, that the ſemen of the male has a certain power to cauſe a greater developement of certain parts. But ſo vague an explanation not ſatisfying me, I have endeavoured to analyſe facts, and from this analyſis I have ſought a ſolution, which might be applicable to the moſt eſſential peculiarities of theſe facts. In a word, I have ſuppoſed that the ſtrong traces of reſemblance between the mule and aſs, implied in the ſemen of the latter, ſomething more than a ſimple ſtimulating power: am I deceived,

ceived, think you, in this conclufion, and are you inclined to believe, that a fimple ftimulating power is fufficient for the whole? I cannot yet prefume fo much, but it is very poffible, that a more fatisfactory hypothefis than mine may be imagined, and I will be the firft to adopt it.

CLXVII. You have done every thing you poffibly could, in order to detect the real nature of the femen of your amphibious animals. It is not then either vifcous or inflammable, acid or alkaline; and yet how wonderful is its energy! It evaporates like water, and it is very well worth remarking, that its moft volatile part is precifely that which is unfit for fecundation. This part, in all appearance, is only lymph, or rather fimple ferum, appointed to prevent the too great vifcidity of the fecundating part. It would be an interefting employment to carry on thefe refearches to the femen of large animals: they have not been pufhed fo far as they ought. Nor would it be lefs interefting to know, whether the femen of large animals incorporated like that of your amphibia, with a great quantity of water or other liquors, would in like manner preferve its energy. The happy experiment made on your bitch, points out the path that fhould be followed in order to afcertain this point. The feed has been conftituted in a fecret relation to the nature of the power which caufes irritability, fince it is deftined to increafe the operation of this power; but we have not yet fufficient knowledge of the nature of irritability. I would not, however, venture to affirm, that there does not exift exist in nature some other liquor besides semen, capable of causing the evolution of the germ. Who knows, if the powder of the stamina of certain plants, may not make some impression on certain germs belonging to the animal kingdom? This is, if you please, a silly idea, but I lay before you every thing which passes through my brain. I could wish, that the powder of the stamina of the barberry might be tried, in which their fetid and penetrating smell seems to announce great energy. Animals and vegetables compose but one family, and their analogies are very numerous. Inverted experiments of this nature ought to be attempted, for it is only by infinitely multiplying the combinations of beings that our knowledge increases. I am always a little mistrustful of our general conclusions, however apparently well founded, because our premises are always more or less particular.

Thus, my dear friend, I have gone over all the paragraphs of your excellent letter; and I wish my reflections may afford you satisfaction.

I began this long letter the 9th of December, and it has employed me till this day, the 10th of January. If you send me any account of your new experiments on the generation of plants, I shall be able to make use of it at the end of Part the Xth, provided it does not come too late.

My wife was very sensible to the politeness of your obliging recollection of her. She desires me to present her compliments and congratulation on the success of your inquiries. Her health is rather better this winter than it was the laſt; but returns of the cholic, from time to time, make her ſuffer ſeverely.

You and the univerſity have loſt a great protector: but I am certain, that her ſucceſſor will patronize the arts and ſciences. The Guardian Angel of Auſtrian Lombardy, Count Firmian, has ſuſtained a great loſs, and his feeling heart will be deeply ſenſible of it. Preſent to that Sage freſh aſſurances of my reſpect and unfeigned wiſhes for his welfare. Accept yourſelf the vows which I am inceſſantly offering for you, my dear and celebrated friend, and entertain as much eſteem for me as I feel for you,

The Contemplator of Nature.

END of the LETTERS.

A DIS-

A

DISSERTATION

CONCERNING THE

GENERATION

OF

CERTAIN PLANTS.

CHAP. V.

GENERATION OF THE PLANTS, DENOMINATED BY LINNÆUS SPARTIUM JUNCEUM, VICIA FABA, PISUM SATIVUM, DOLICHOS UNGUICULATUS.

I. WHOEVER is acquainted with Natural Hiſtory cannot be ignorant, that the three principal ſyſtems reſpecting the generation of animals, the ſyſtem of the *ovariſts*, that of the *vermiculiſts*, and that founded upon the *two liquors*, have been transferred, with the neceſſary modifications, to plants. Some think, that the embryos pre-exiſt in the ovarium, others that they are tranſported thither in the impregnating powder, and others believe, that they are generated in the ovarium, by the combination

tion of two fecundating principles, the one furniſhed by the piſtil, the other by the ſtamina.

My chief purpoſe being to inveſtigate the generation of certain plants, I conceive there can be no better way to arrive at truth, than to fix my attention chiefly upon the ovarium. That I might have the greater chance of ſurpriſing Nature in her operations, I determined to examine this organ at three different periods; before fecundation, at the time it takes place, and after it has been effected; or, what amounts to the ſame thing, after the aſperſion of the pollen. I was therefore obliged to examine the flowers while they were yet cloſed, when they were in full bloſſom, for that is the ſeaſon of impregnation, and after the petals had dropped. I begin with the ſpecies denominated by Linnæus and others, *Spartium Junceum*, Ruſhy-twigged broom.

II. From the nature of this plant, as well as of others without number, the flowers of the ſame branch are not all equally forward, ſome being in the ſtate of a ſmall bud,—theſe are ſituated higheſt on the branch; others already blown, or about to blow,—theſe occupy an intermediate ſituation; and others again fallen or falling,—theſe grow on the loweſt part. The ſame branch therefore furniſhed matter for various obſervations. The ſmalleſt buds were firſt to be examined. They are perfectly compact, and form a ſolid body, ſcarce a line in length. If they be dexterouſly opened with fine inſtruments, the petals may be diſentangled and brought into view. They are of a light green, without any tinge of of yellow, which is the colour of the ripe flowers. When the petals are removed, the ſtamina and piſtil, the organs of generation, come into view. The powder of the antheræ may be perceived; it is far from being ripe, as is evident, not only from the extreme minuteneſs of the granules of which it is compoſed, but from its want of volatility, a property it eminently poſſeſſes when mature. It is now fixed to the antheræ by means of a viſcid matter. The piſtil, extremely tender, ariſes from the middle of the flowers. If its baſe be freed from the ſurrounding teguments, and attentively examined with a glaſs, the pod may be ſeen formed about $\frac{1}{10}$ of a line in length. If the pod be examined externally, ſeveral tumours may be ſeen along the ſides, which, when obſerved againſt the light, are found to be produced by granules lodged within the pod, or, as I ſhall call it, the ovarium. Upon opening the ovarium longitudinally, theſe granules are found to be ſeeds, of very ſmall ſize and round ſhape; they are diſtributed in their natural order, in ſo many depreſſions or ſockets, and attached by filaments (*appiccature*) to the inſide of the ovarium, juſt as the ripe ſeeds are in the ripe and dry pods of the plant. Theſe ſeeds are not found, upon diſſection, to conſiſt of an external cover, and a nucleus compoſed of two lobes, with a germ or plantule, as in their mature ſtate; but they exhibit an apparently homogeneous ſubſtance, ſpongy, and like a tender jelly. From the reaſonable ſuppoſition which I formed, that theſe are the ſeeds, it follows, that they exiſt in the ovaria at leaſt twenty days before the flower

is

is in full bloom, or in other words, before fecundation. Flowers at leaſt of equal ſize of another branch, growing from the green ſtem of the ſame plant, were not full-blown before the twenty-fifth day.

III. The diſſection of ſeveral buds, larger by about one-half than the preceding, preſented the following phænomena. The petals, which were beginning to aſſume a yellowiſh hue, were not now ſo cloſe and compact; and the fecundating powder ſtill adhered to the ſummits. The ſtamina were become longer and thicker, as alſo was the piſtil. The ovarium was not ſo ſmall, and the ſeeds had grown in proportion; their ſubſtance was not ſo gelatinous, though it continued ſtill homogeneous and ſpungy.

In flowers a little further advanced, the only difference conſiſted in an increaſed ſize of the petals, ſtamina, piſtil, and ſeeds. Nor was there any eſſential change when the petals were expanded, and the powder of the ſtamina, being now mature, might be ſhaken from the antheræ by the ſlighteſt agitation, and diffuſed itſelf in a cloud through the air. In the ſeeds I could not at this period diſcover either lobes or plantule, but they were of a greeniſh colour and uniform ſubſtance, which was ſpungy and full of moiſture. Yet I could diſtinguiſh the lobes and plantule in the ripe ſeeds contained in thoſe pods, which had acquired a black colour, and were grown dry. It was therefore neceſſary to infer, that the two lobes and the plantule are either generated, or rendered viſible during the ripening of the ſeeds. Hence, in order to diſcover the generation or the appearance of theſe parts, I was

was obliged to continue my obſervations till I had gained ſome ſatisfaction upon this curious and intereſting point.

I began with the ovaria or pods, from which the flowers had fallen ſome time. In ten days afterwards there was no difference. On the eleventh ſome new appearances began to take place in the ſeeds. They were no longer round, but reſembled an heart, of which the baſis was attached to the pod by an appendix, and towards the apex, when gently compreſſed, there appeared a whitiſh point in motion. When the heart was cut open longitudinally and the inſide inſpected, this white point proved to be a ſmall cavity, incloſing a drop of liquor, which had been made to move by the preſſure of the fingers.

Twenty-one days after the pods were ſtripped of their flowers, the cavity, which at firſt appeared at the apex, was enlarged, and extended much farther towards the baſe; it was full of a tranſparent liquor, with which the ſpungy ſubſtance of the ſeeds was alſo moiſt. On the twenty-fifth day the cavity was more enlarged, and ſtill full of liquor; it moreover contained a very ſmall ſemi-tranſparent body, of a yellowiſh colour, gelatinous, and fixed by its two oppoſite ends to the ſides of the cavity.

In a month the ſeed was much enlarged, and its ſhape is changed from a heart to a kidney; the little body contained in the cavity is increaſed in bulk, is become leſs tranſparent and gelatinous, but there appears no ſign of organization.

On the fortieth day the cavity, now grown larger, is quite filled with the body, which deſerves

deserves to be more particularly examined. It is surrounded by a thin membrane, somewhat viscid and tender; after this membrane is removed, the body appears bare, and without any other teguments; it is of a bright green colour, and may easily be divided by the point of a needle into two portions, in which are manifestly recognized the two lobes; within these we may easily perceive the plantule exceedingly small, and attached to the lower part. The lobes, together with the membrane, are afterwards defended by a husk or skin, which forms the outside of the seed.

The reader will easily guess what afterwards happened to the pods as they grew riper. The lobes and the plantule were only more and more developed, and by degrees acquired greater firmness. And in this manner did the seeds of the broom arrive at maturity.

V. The foregoing observations shew, 1. That the seeds of this species exist in the ovarium many days before fecundation. 2. That they remain for some time solid, and then a cavity, containing a liquid, is formed in them. 3. That after fecundation a body begins to appear within the cavity, fixed by two points to the sides; and when in process of time it has attained a larger size, it proves to be the two lobes inclosing the plantule. 4. That the ripe seed consists of two lobes adhering to the plantule, and surrounded by a thin membrane, which is itself covered with a husk or cuticle.

These deductions illustrate the generation of the plant in question. We learn, that the embryos do not appear till after the falling of the flowers, and consequently not till after fecundation, though the seeds, or, to speak more properly, the integuments of the seeds may be seen long before.

VI. Having analysed the fructification of this species of broom, I proceeded to that of the common bean, *Vicia Faba*. I began with those which had the smallest blossoms. They appear externally of a green colour, and likewise internally, if the tender petals be unravelled; here and there, indeed, the green approaches to a white. The stamina are visible; the antheræ, instead of pollen, exhibit a viscid gelatinous substance. The pistil is of a white and green colour; towards the apex it is villous, and within the base or ovarium, if it be inspected against the light, may be perceived the seeds. It may be remarked, that the ovarium of the bean more easily splits into two than that of the broom, and that the seeds of the former plant are in like circumstances larger than those of the latter. The seeds of the bean are roundish, but on one side there is an hooked and sharp beak. They are semi-transparent, gelatinous, and when opened, are found to be solid and without a cavity.

If blossoms not quite so small be examined, the powder of the stamina is perceptible; it is found to be imbedded in a glutinous substance. The ovarium having now attained a larger size, contains seeds proportionally increased in size; they are neither so gelatinous nor so transparent, but they contain no cavity. Other seeds further advanced, and belonging to blossoms about to open, or already expanded, are in the same state.

The

The cavity is not viſible till the 3d or 4th day after the falling of the flowers; it is then found to be full of a tranſparent liquor. The ſeeds and the cavity increaſe together, and fifteen or twenty days after the fall of the petals, the cavity contains a body, which, when magnified, ſeems to be ſplit into two, though the obſerver cannot be certain of this till afterwards; when if he opens ſeeds nearer maturity, he will find them to divide very eaſily into two portions. Theſe portions are manifeſtly the two lobes, within which is contained the tender naſcent plantule. If the lobes and the plantule be treated with extreme care, a mucous filament is found attached to the plantule, which, after paſſing between the lobes, is implanted in the ſubſtance of the little ſeeds. The lobes and the plantule are merely developed in proceſs of time. They are covered, as in the abovementioned broom, with the integuments of the ſeed.

VII. I ſhall relate at once my obſervations upon peaſe and *kidney-beans*, the appearances they offered being perfectly alike. The ſeeds are found in the ovarium full formed, when the flowers are little advanced, and when the buds are yet ſcarce viſible. The ſeeds now ſeem to conſiſt of an homogenous ſubſtance, and are without any internal cavity. The ſame may be ſaid of them when they are become many times larger, and when the powder of the antheræ is ripe. The cavity begins to be viſible ſome days after the withering of the flowers, and in ten or twelve days more it is enlarged, and a white mucilaginous point connected by a ſlender filament

filament with the ſeeds may be perceived in it. This point is the rudiment of the lobes, between which the plantule may be diſtinguiſhed by the aid of the microſcope. What follows may eaſily be imagined: the lobes and plantule increaſe in ſize, and their increaſe implies the growth of the ſeeds which contain the lobes.

If we compare theſe obſervations upon kidney-beans and peaſe with the foregoing upon broom and beans, we ſhall find, that they all contribute to prove that the ſeeds, or their integuments, exiſt before fecundation, but that the plantule and the lobes cannot be ſeen till afterwards.

It would therefore appear, that theſe reſults do not agree with my obſervations on amphibious animals; for in them I found the fetuſes before impregnation. But are we immediately to conclude, that in the proceſs of generation Nature follows two different methods in plants and in animals? The principles of ſound reaſoning do not permit me to make ſo haſty a declaration; they require, that I ſhould carry further my reſearches on plants. Theſe reſearches I undertook, and a relation of part of them ſhall be the ſubject of the following chapter.

CHAP. II.

GENERATION OF THE PLANTS DENOMINATED BY LINNÆUS, RAPHANUS SATIVUS, CICER ARIETINUM, IXIA CHINENSIS, DELPHINIUM CONSOLIDA, CUCURBITA PEPO, CUCUMIS SATIVUS. EXAMINATION OF THE POWDER OF THE STAMINA.

VIII. *Raphanus ſativus, common Radiſh.* The ſeeds are viſible in the ovarium long before the powder of the antheræ is ripe. They are ſolid, and continue ſo till twelve or fifteen days after the falling of the flowers: the uſual cavity then appears, of ſmall dimenſions at firſt, and full of liquor, but ſucceſſively enlarging, inſomuch that about the 20th day, it occupies almoſt all the inſide of the ſeeds. In three or four days more, the lobes are ſeen ſurrounded as uſual by a tranſparent liquor; and we may ſoon afterwards perceive between them a mucilaginous filament, attached by one end to the plantule, and by the other to the internal cavity of the ſeeds. When the ſeeds are nearly ripe, the two lobes are conſiderably increaſed; they reſemble a pear with its ſtalk; the ſtalk is implanted into the concave ſurface of the ſeeds. If the lobes be taken out and carefully examined, the ſtalk appears to be part of the plantule; and inſtead of two lobes, the number obſerved in the plants before mentioned, there are four; the two ſmaller

ſmaller are implanted in the two greater; they are all united, and ſo attached to the plantule, as with it to form one body.

IX. *Cicer arietinum*, Chick-pea. The piſtil in this plant repreſents in miniature a gourd, of which the belly is the baſe of the piſtil, or, as I call it, the ovarium. If this be opened longitudinally twelve or fifteen days before fecundation, two pointed ſeeds, ſolid and of a green colour, come into view. As the parts of fructification ripen, the ovarium ſwells and takes the form of a purſe, to which the ſeeds are attached. About four or five days after fecundation, the cavity begins to form, and on one ſide of it may be perceived two very minute lobes; if they be carefully opened, the plantule is ſeen attached to the ſeeds by a mucilaginous filament. Nothing further takes place except a progreſſive evolution.

X. *Ixia chinenſis*. Having opened longitudinally the ovarium of one of the ſmalleſt buds, which is oblong and prominent at the centre, we find the pyriform ſeeds diſpoſed in their natural order, and perfectly ſolid within. The ſame thing may be obſerved when the petals are opened, nay, after they are fallen, and ſtill nine or ten days more elapſe before the cavity begins to open. This plant is diſtinguiſhed by a peculiarity which muſt not be omitted. At firſt the cavity is full of a tranſparent liquor, which in time thickens, and at length is converted into a white gelatinous matter, not unlike coagulated milk. The conſiſtence becomes gradually firmer, and before the ſeeds are ripe, reſiſtance is felt on attempting to cut it with a knife.

knife. This ſubſtance, thus hardened, occupies the whole cavity of the ſeeds, nor can there be perceived the ſmalleſt veſtige of the lobes. My obſervations were made upon ſeeds not yet arrived at perfect maturity, but I could not continue them for want of riper ſeeds. Are we to ſuppoſe that the ſubſtance, which is at firſt liquid, then gains ſome conſiſtence, and at laſt becomes hard, performs the office of lobes, ſo that they are produced in this inſtance in an unuſual manner; or is it more probable, that the lobes appear later than in other ſeeds? The phænomena of another plant, of which I am now to ſpeak, incline me rather to the latter opinion.

XI. *Delphinum conſolida*, Larkſpur, ſubdivided larkſpur. The appearances noticed during the growth of the ſeeds of the Ixia, recurred on the preſent occaſion. The cavity, which is uſually formed after the falling of the petals, is ſoon filled with liquor, at firſt tranſparent, then whitiſh, and afterwards of the colour and conſiſtence of milk. If the ſeeds be now boiled for a ſhort time, the milky liquor is changed into a matter like pomatum. There appears, upon examination, no ſign of organization. When the ſeeds are nearly ripe, the conſiſtence of the white liquor is increaſed, and when they are arrived at maturity, its hardneſs is conſiderable. The plantule and the lobes may now be diſtinguiſhed, but they are ſo buried in the midſt of this indurated matter, that it is difficult at firſt to diſtinguiſh them certainly, as well on account of their minuteneſs as their white colour. This obſervation, which agrees ſo nearly with the preceding, leads me

to

to ſuſpect, that the milky liquor of the *Ixia*, which is firſt coagulated and then hardened, does not coſtitute the lobes, but only encloſes and conceals them. They are inviſible, either on account of their minuteneſs, or becauſe the ſeeds are not yet ripe. In the preſent ſpecies, the late appearance of the lobes, which does not takes place in much leſs than a month after fecundation, is very remarkable (*a*).

XII. *Cucurbita Pepo*, Common Pumpion or Pumpkin. Diſregarding for the preſent the male flowers, as having no relation with the preſent inquiry, on account of their ſterility, I directed my attention to the female flowers, beginning with the bloſſoms that were leaſt advanced; ſmall, however, as they are, the little pumpion is already formed under the bud, and the ſeeds within it are very viſible. In this plant there is a peculiarity in their ſtructure. They are not compoſed of an homogeneous ſubſtance, as the unripe ſeeds of other plants, but conſiſt of two matters, a ſhell and a nucleus. When the ſeed is cut tranſverſely, theſe two ſubſtances

(*a*) I found, by obſervations made a year afterwards, on more advanced ſeeds of the Ixia, that my ſuſpicion was juſt. When a longitudinal ſection is made along ſuch ſeeds, the plantule is very viſible; it has the ſhape of a cone; the baſe lies at the bottom of the ſeed, and the apex reaches about as high as the middle. The whole plantule is imbedded, and hid in the milky matter, which at this period is very hard. I muſt moreover obſerve, that notwithſtanding all my pains, I could not diſcover the lobes; perhaps the part I call *plantule* might ſerve in their ſtead. The appearance of the plantule is as late in the ſeeds of the Ixia, as in thoſe of the larkſpur.

 appear,

appear in the plane of fection; if a portion of the divided feed be held by the flat fides between the finger and thumb, and preffure be made towards the cut, the nucleus quits the hufk, and flies into the air, as the ftone of a cherry does when preffed in like manner. If the nucleus be taken whole out of its cover, it appears in the fhape of a pear with its ftalk, and bears, moreover, a perfect refemblance to the lobes with the plantule. But are we to conclude, that the plantule is really vifible here before fecundation? I confefs, this firft obfervation on the fructification of the pumpkin, was very near leading me to adopt this opinion. But one circumftance fuggefted a doubt. In other plants the lobes are eafily feparable into two, and fometimes into four (VIII); in the prefent cafe, the nucleus would not admit of fuch a feparation, but formed a perfect whole. The beft way of removing my perplexity, evidently was to profecute my obfervations. From the examination of three larger buds I learned, that the nucleus yet confifted of one body, and that it adhered fo firmly to the cover, that it could not at all, or at leaft not without great difficulty, be parted from it. But the cover or rind appeared much more complex than I had at firft conceived. It confifts of three diftinct membranes. The firft or outermoft is the moft thin and delicate of all; it may be denominated the cuticle or epidermis. The fecond, which lies immediately under the firft, is thicker, and of a whitifh colour, and has a degree of hardnefs approaching to that of wood; there is fome difficulty in fepa-

rating

rating it from the third, which invests the nucleus; this innermost skin or membrane is greenish and firm, but not so firm as the second.

The inspection of the seeds, twenty-five days after the falling of the flower, gave me an opportunity of observing other new circumstances. The nucleus, which so much resembled the lobes, had disappeared, and in its stead, there was a membranous sac, terminated by a beak, that was attached to the inside of the seeds. The sac was somewhat tumid, with a liquid stagnating within it, as became evident whenever it was perforated. By gently laying hold of the beak with a pair of forceps, the little sac may without difficulty be entirely removed. It is composed of two membranes, which, when opened, are found to contain a very tender mucilaginous body full of moisture. This body arises from the inside of the apex of the seed, and extends about half its length; it is attached to the place at which it terminates. The slightest touch breaks and destroys it. When examined by the microscope, it appears reticulated, whence I conclude that it is organized.

In seeds further advanced, when the flowers have been withered above a month, the mucilaginous body enclosed in the membranous sac is much enlarged, and at the inside of the apex of the seed appears a white, solid, and conical particle, which, when attentively examined, appears to be the germ or the plantule in miniature. To the plantule are attached the two lobes like it, of a white colour, and exceeding it in size; the mucilaginous filament above-mentioned passes in the middle between them, and is inſerted into the plantule. It is eaſy, from what has been ſaid above, to gueſs how the ſeeds gradually advance towards maturity. The mucilaginous filament enlarges in its dimenſions, the plantule increaſes, and the two lobes advancing in bulk and maſs, come at laſt to occupy almoſt all the inſide of the ſeed. It appears, therefore, that I ſhould have been greatly deceived if I had taken the nucleus, which is found within the ſeeds before the opening of the flowers, for the lobes and the plantule, ſince theſe parts do not make their appearance till a month after the withering of the petals.

XIII. *Cucumis ſativus*, Cucumber. My obſervations on the ſeeds of this plant, ſo exactly reſemble thoſe on the ſeeds of the pumpion, both with reſpect to the appearance of the nucleus before fecundation, the membranous ſac, and the gelatinous ſubſtance, which are obſerved afterwards, and laſt of all the lobes and plantule, that I think it unneceſſary to enter into a particular account of them. I ſhall only ſay, that in the cucumber the plantule and the lobes ſeem to me to become viſible ſtill later: they at leaſt were very ſmall in one inſtance, when the fruit was nearly ripe, and beginning of courſe to turn yellow.

I afterwards proceeded to examine other plants, *Hibiſcus Syriacus*, the Syrian ſhrubby Mallow, *Alcea roſea*, roſe-flowered, or common Hollyhock, *Acanthus mollis*, ſoft or ſmooth-leaved Acanthus, *Convolvulus purpureus*, purple Bindweed, to which may be added *Ocymum Baſilicum*, ſweet Baſil, *Cucurbita*

bita citrullus, Cannabis ſativa, Hemp, *Mercurialis annua*, annual Mercury. All theſe plants perfectly agree in the appearance of the ſeeds long before fecundation, and of the plantule and lobes, ſome time after the ripening and diffuſion of the pollen. This general obſervation perfectly coincides with the account of the great naturaliſt Duhamel, given long ſince in his *Phyſique des Arbres*. Speaking of the formation of the ſeeds of trees bearing fruit with ſtones, ſuch as the almond, th epeach, plum, cherry, &c. he adduces many facts to prove, that when a ſtone, which has attained its full ſize, is opened before the fruit is ripened, (and we may make the experiment a long time after fecundation), it will be found full of a viſcid liquor; a white body, he ſays, begins to appear within the point of the kernel, encloſed in a ſmall tranſparent bladder: this body is the rudiment of the lobes with the plantule; they grow till they fill the whole capacity of the ſtone. From the coincidence of ſo many facts, I think myſelf authoriſed to infer, that this law is obſerved by Nature, if not in all plants, yet in a great number.

XIV. But with which of the three theories reſpecting the generation of plants (I), do my numerous obſervations beſt agree? My principal aim was, as I have already remarked, to find, if poſſible, data ſufficient for the ſolution of this difficult problem. Should we be content with firſt appearances, we might perhaps think that ſyſtem preferable, according to which, the embryos of plants are conveyed, at the time of fecundation, by the powder of the antheræ into the ovarium.

The

The embryos have never been found in this organ before the aſperſion of the powder. They always make their appearance after the pollen has been obſerved upon the antheræ. It ſhould therefore ſeem, that the exiſtence of the embryos directly depends upon this powder; and it may be inferred, with ſome degree of probability, that the embryos preexiſt in the pollen; and that when it falls upon the ſtigmata, they are conveyed through particular ducts into the ovarium. A learned naturaliſt (*a*), has accordingly been led to adopt this opinion, by the obſervations of ſome perſon, who pretended to have ſeen the embryos in the ovarium, immediately after the ſummits had diſcharged their fecundating duſt. But whoever conſiders, with a little attention, the force of the argument, will find, that it is not very convincing. It is in the firſt place not true, that the embryos may be perceived as ſoon as the powder has fallen from the ſtamina. They do not come into view for more than a week, and ſometimes not till a month after the diſperſion of the powder, as is evident from all the obſervations I have hitherto related, and from others which I ſhall hereafter relate. In the ſecond place, this mode of reaſoning is, as Mr. Bonnet has very well remarked, illogical; when we infer, that the embryo does not exiſt in the ovarium before fecundation, becauſe it is not viſible before that time, we argue from inviſibility to nonexiſtence, a fallacious way of reaſoning, as

(*a*) Needham: Nouv. decouvertes faites avec le Microſcope.

many

many inſtances may be brought to ſhew: in the impregnated egg, for example, the beſt microſcopes will not bring the chicken into view, though we are ſure that it is preſent. Notwithſtanding the inconcluſiveneſs of the argument, there ſeems no abſurdity in ſuppoſing the embryo of the plant to pre-exiſt in the powder, and thence to paſs into the ovarium. To remove theſe uncertainties, I reſolved to give my inquiries a different direction.

XV. If the embryos of plants be really lodged in the pollen, why cannot we diſcern them by microſcopes of high magnifying powers? The thing did not ſeem improbable, and I thought it would be proper to turn my reſearches to this object. I accordingly examined the ripe pollen of ſeveral plants, and eſpecially of thoſe upon which the foregoing obſervations were made. In the flowers of theſe vegetables, I found the powder to reſemble very nearly that of others examined by botaniſts, and in particular by Duhamel; it formed an aggregate of particles, differing in figure in different plants; in ſome the particles are round, in others elliptical, in others pyramidal or conical, in others of an irregular form. Sometimes they are ſmooth, ſometimes prickly, and at others covered with knobs. They are in different plants tranſparent, opake, yellow, white, blue, or carnation. The ſize varies according to the difference of the flowers. But theſe corpuſcles are not, according to the common conſent of Naturaliſts, immediately concerned in the generation of plants. They are veſicles, ſheaths, caſes, —, or by wha-

whatever name we may chuſe to diſtinguiſh them, for the moſt part full of a thin liquor, which, when they are moiſtened, they emit with force. In this liquor floats a multitude of very ſmall globules, which, at the time of exploſion, are thrown in different directions. Thoſe who adopt the theory we are conſidering, ſuppoſe the rudiments or embryos of the plants to be lodged in this liquor. It was therefore proper to fix my attention upon it, and to try whether I could perceive any thing reſembling an embryo. When the embryos are firſt ſeen in the ſeeds, they repreſent a ſmall pointed body, to which two larger bodies are affixed, like the cloſed wings of a butterfly; theſe are the lobes. I examined the liquor with great attention, in order to ſee whether it contained any ſuch bodies, or any thing like them. I uſed glaſſes of various powers, from the loweſt to the higheſt; but I could diſcover nothing except the liquor, which in ſome caſes is like oil or melted fat, and the globules that uſually float in it. As the liquor, a ſubſtance without organization, cannot be what we term embryos, I had only to examine the globules; but I could perceive nothing in them, which bore any reſemblance to lobes or embryos. Theſe, as the word *globule* imports, are round bodies, or ſuch as have nearly that figure; they conſtantly retain it with whatever microſcopes they are examined. Nor did their form change, when I uſed the ſolar microſcope, which ſo prodigiouſly enlarges objects. Of theſe facts, it is the direct conſequence, that we cannot aſſert, with any proba-

probability, that the embryos of plants are lodged in the pollen.

It may, I am well aware, be objected, that the globules are the real embryos, and retain their round ſhape as long as they continue in the powder; but that as ſoon as they paſs into the ovarium, and find a proper ſupply of nouriſhment, they unfold the parts which were before concentrated in a minute globe, and aſſume the ſhape of the lobes and plantule; juſt as the tadpoles of the frog and toad, according to my obſervations, repreſent ſo many globules as long as they remain in the body of the female, but when they come to be nouriſhed with the liquor of the amnion (*a*), they put on the real form of the tadpole. Such an objection may be ſtarted, but it amounts to no more than a bare poſſibility, that the globules floating in the liquor of the pollen may be the embryos. But I am not ſeeking for poſſibilities; I am inquiring whether the ſuppoſition is probable; whether there is any juſt motive for ſuppoſing the embryo to pre-exiſt in the powder of the ſtamina: and my obſervations ſhew not any ſuch probability, or any ſuch juſt motive. Not having, by theſe means, obtained the information I ſought, there remained one other chance of ſolving the problem, by interrogating Nature in a different manner. This was to try, whether the embryos would appear and grow to maturity, when the powder of the antheræ was hindered from acting upon the piſtils. Should the event be ſuch, it would be evident, that

(*a*) Diſſert. I.

the

the embryos do not belong to the pollen, but the ovarium. The experiment might be made in three ways, either by cutting away the antheræ of hermaphrodite flowers, before they ſhed their powder upon the piſtil; or by removing the male flowers of thoſe plants, in which they grow ſeparately on the ſame individual before they are ripe; or laſtly, by keeping the male plants at ſuch a diſtance from the female, that there can be no ſuſpicion of their duſt reaching them; this experiment relates to plants producing two ſorts of individuals; one provided with ſtamina only, the male, and the other with piſtils only, the female. Theſe experiments, together with ſome obſervations, will furniſh matter for the two following chapters.

CHAP. III.

GENERATION OF SOME HERMAPHRODITE AND MONOICOUS PLANTS, UPON WHICH THE POWDER OF THE STAMINA WAS PREVENTED FROM ACTING, CALLED BY LINNÆUS OCYMUM BASILICUM, HIBISCUS SYRIACUS, CUCURBITA MELOPEPO FRUCTU CLYPEIFORMI, CUCURBITA CITRULLUS.

XVI. I BEGAN with an hermaphrodite plant, viz. that ſpecies of Baſil which is often raiſed in pots, and called by botaniſts *Ocymum Baſilicum*, ſweet baſil. I happened, at at the time I conceived the deſign of entering upon this inquiry, to have ſome in blow, and others of which the flowers were not yet expanded. That I might form a juſter judgment of my experiments, I reſolved to compare flowers, in which the pollen had acted with others, in which its influence had never been exerted. My obſervations upon the former were the following: every bloſſom of baſil had four ſtamina, in the midſt of which riſes the piſtil; the ſtamina, as well as the piſtil, are diſtinctly viſible twelve days and more before the opening of the flower, when the petals have been cautiouſly removed. The ſeeds appear at the ſame time in the ovarium; they are four in number, and have an oval ſhape. They continue ſolid for ſeveral days after the falling of the flowers, but ſoon afterwards, while they are increaſing in bulk, a cavity, like that which I have obſerved in the ſeeds of other plants, begins to form. Within this cavity, we may in a ſhort time diſcern a ſolid point, at firſt ſhapeleſs, but ſoon putting on the form of the lobes and plantule. Meanwhile the cavity becomes more capacious, as the plantule and lobes grow, and at length theſe bodies occupy the whole inſide of the ſeeds. Such is the proceſs of Nature in thoſe flowers of baſil, in which the powder of the ſtamina is left to produce its effects: we have ſeen that the ſame phænomena occur in other plants.

XVII. It is now proper to proceed to the other more intereſting part of our comparative inveſtigation, and to obſerve what becomes of the embryo, when the influence of the pollen is withdrawn. At the time when the flowers were about to open, I cut away

away the four antheræ, which now were full of ripe pollen. But this operation alone did not ſatisfy me. The exceſſive tenuity of that halitus, which burſts from the pollen (XV), and which, in the general opinion of botaniſts and naturaliſts, has an immediate effect in generation, (in whatever manner it may produce this effect) rendered it neceſſary not only to remove the pollen, by cutting away the antheræ of thoſe flowers upon which the experiment was made, but alſo to keep at a diſtance the pollen of other flowers growing on the ſame plant. This end was eaſily obtained, by plucking off all the flowers as they were about to be expanded, except thoſe which were the ſubject of my experiment. The pot was placed on a window, and there grew near it no other plants of the ſame kind, either in pots or in the ground. I cannot relate what happened without aſtoniſhment. The ſeeds, ſome time after the falling of the flowers, became hollow within, and in the cavity appeared the lobes and the plantule, which grew till they filled the whole capacity of the ſeeds. In ſhort, the various phaſes aſſumed by ſeeds that have been ſubjected to the influence of the pollen, were obſerved without any difference in others, which may reaſonably be ſuppoſed never to have felt that influence.

XVIII. I could not however acquieſce in this reſult: but was rendered uneaſy by a ſuſpicion, that ſo delicate an experiment had not been executed with all the neceſſary precautions. According to the common opinion of philoſophers, the powder of the ſtamina exerts its action while the flowers are open-

ing.

ing, or ſoon after they are blown. But on opening the flowers of baſil two or three days before their time, I thought that in two or three inſtances part of the pollen was ripe, and that one or two grains had fallen upon the piſtil, which lies very cloſe to the antheræ, and indeed almoſt touches them. I therefore ſuſpected, that I had cut away the antheræ too late, and after the pollen had produced its effects. This conſideration led me to perform the operation upon flowers leſs forward, and not liable to ſuch a ſuſpicion. And now the event was different. I made the experiment upon eighty-four bloſſoms. The ſeeds of about one-third of this number fell before they were ripe. In the ovaria of the reſt appearances were exceedingly various. Some ſeeds, notwithſtanding it was the ſeaſon of their maturity, were as ſmall as when the antheræ were cut away. In ſome ovaria they were larger, but withered, dry, or quite ſpoiled. In twenty-five inſtances, they were as well looking and as large as thoſe which had been ſubjected to the influence of the pollen. I was not however to be ſatisfied with external appearances. The principal queſtion was, whether they each contained an embryo, or a plantule attached to lobes? They actually contained theſe parts, nor could I, by analyſis or compariſon, perceive any circumſtance in which they differed from the plantule and the lobes of ſeeds, upon which the influence of the pollen had been exerted. Of theſe twenty-five ſeeds, certainly never acted upon by the powder of the antheræ, twelve were analyſed in this manner, and the remaining thirteen were put into the ground. But they did not germinate, while thirteen ſeeds of the ſame plant, left to the action of the pollen, and ſown in the ſame place, all came up. The immediate and moſt ſtriking inference to be deduced from theſe obſervations is, that the fecundation, and ſometimes the evolution of the embryos of baſil, depend upon the pollen, but not their exiſtence.

XIX. Whilſt I was thus employed upon baſil, I made obſervations and experiments of the ſame nature upon Syrian ſhrubby mallow, *Hibiſcus Syriacus*. The antheræ were cut away from a great number of bloſſoms before they expanded. I could not however, I muſt own, be certain, that a grain or two of powder had not fallen upon the piſtil; for I oftener than once obſerved, that it was quite ripe. Some ſeeds fell ſoon after the operation, but the greater part remained in their proper ſituation, and grew conſiderably; they would perhaps have arrived at maturity, if the coolneſs of the atmoſphere at the ſetting in of autumn, had not prevented them, as it prevented other ſeeds of the ſame ſpecies, which were left to themſelves, and muſt be ſuppoſed to have been impregnated by the pollen. Some of theſe ſeeds, which had grown a good deal, but were yet unripe, were black within and ſpoiled, others were white, in all appearance ſound, and, what is of chief conſequence, they contained a plantule attached to its lobes, which were convoluted like cut lettuce. From the plantule aroſe, in ſome caſes, an almoſt imperceptible gelatinous filament, of which the other end was inſerted into the ſeeds.

When

When I cut off the antheræ from the blown flowers, I performed the ſame operation upon others ſeveral days before they were to expand. To effect my purpoſe it was neceſſary to force open the bud, which could not be done without ſometimes injuring a petal. At this time it ſeemed certain, that the pollen could not have produced any effect, as it was viſcid, and adhered tenaciouſly to the ſummits of the ſtamina. The reſults exactly correſponded with the foregoing obſervations. The ſeeds of ſeveral flowers dropped prematurely, while in others they continued firm, and grew conſiderably, but did not ripen: ſome were vitiated internally; others contained lobes and a plantule completely developed and ſound.

It is obvious, that the conſequences of the experiments on ſhrubby mallow and baſil perfectly agree; they equally concur to prove, that although the due evolution of the embryos depends on the action of the pollen, this powder neither conveys nor produces them.

XX. Having ſufficiently ſatisfied my curioſity, with reſpect to theſe hermaphrodite plants, I proceeded, according to the plan laid down in Sect. XV. to plants bearing male and female flowers ſeparately on the ſame individual. My obſervations were made upon two of this claſs, the pumpion with ſhield-form fruit, *Cucurbita Melopepo fructu Clypeiformi*, and the citron pumpion, *Cucurbita Citrullus*. I had the ſeeds of the former ſpecies from the public botanic garden at Pavia; they were ſown in the ſpring at Scandiano, a well-known and very pleaſant fort belonging to

 the

the ſtate of Modena, and the place of my nativity: here, during the ſummer and autumnal vacations, I employ myſelf in thoſe experimental enquiries, which are the ſubject and the foundation of my unimportant literary productions. At my arrival there at the beginning of June 1777, two individuals, for I had ordered two only to be raiſed, were juſt beginning to put forth a few flower-buds towards the bottom of the ſtalk. At this early period the male flowers may eaſily be diſtinguiſhed from the female. The former, denominated alſo *barren* by botaniſts, have a ſlender ſtalk; while the ſtalk of the former, where it joins the calyx, forms a tumour, conſiſting of the immature fruit, as is intimated in Sect. XII. I paid daily viſits to theſe two individuals, and very carefully watched the progreſs of both ſorts of flowers. That there might be no ſuſpicion of the pollen exerting any influence upon the females, the males were deſtroyed at their firſt appearance. As fruit, when a ſmall quantity only is left upon a plant, is ſooner ripe, and grows to a larger ſize, becauſe it receives a greater ſhare of nutritious juice, I left on each of my two individuals two flowers only; the buds that made their appearance afterwards were taken away, along with the male flowers. Meanwhile my four gourds grew rapidly; finding that towards the middle of September they had attained the uſual full ſize, I gathered one, in order to inſpect the internal parts. The fleſh was too ſoft, becauſe the fruit was not thoroughly ripe; but in colour, ſtructure, and taſte, it reſembled fruit produced by plants which had their male flowers, and which I examined both before and afterwards at Pavia. The ſeeds were in great number, and, as well internally as externally, were perfectly formed; the lobes, indeed, to which the plantule was attached, did not occupy the whole capacity of the ſeeds, but they had not attained their full ſize. At the end of the month the other three gourds were quite ripe; I therefore gathered them, and put the ſeeds of each into a ſeparate box, that I might be able to examine them at pleaſure. The lobes filled the whole inſide of the ſeeds, and had all the characters of perfect maturity.

XXI. Thus far there is a perfect agreement with the obſervations made on the ſeeds of baſil, which ſeemed, notwithſtanding they were deprived of the efficacy of the pollen, to have acquired the ſame degree of perfection as thoſe impregnated in the uſual manner. But as they did not grow, however perfect they might be in appearance, becauſe they had not been vivified by the pollen (XVIII), I imagined, that for the ſame reaſon the ſeeds of my three gourds would not grow. It was, however, proper to make the experiment. I therefore dried one hundred and fifty in the ſun, and afterwards planted them in three pots, fifty in each, taken from ſeparate gourds. But the lateneſs of the ſeaſon, it being the 10th of October, the conſtant rain, and the coolneſs occaſioned by it, circumſtances unfavourable to vegetation, obliged me to place my pots in a ſtove, which, though it was not heated, was kept warm by a contiguous chimney. The event did not by any means correſpond to my expectation; I took

 it

it for granted, that none of the ſeeds would germinate, and yet they almoſt all came up very well. On the 15th, ten days after they were planted, thirty-ſeven, out of one hundred and fifty, were ſprung up; the number increaſed daily, inſomuch, that on the 8th of November, one hundred and thirty-three appeared above the mould in the pots. I was unwilling to leave the ſeventeen ſeeds that did not ſhoot unexamined; I found that they were either empty, or that the lobes and the plantules were vitiated; that is to ſay, criſp and withered, becauſe they were unripe.

I reſerved the remainder of the ſeeds for another experiment to be made the following ſpring. Before it can be aſſerted that fructification has been complete, it is neceſſary, according to the determination of botaniſts, not only that the ſeeds ſhould grow, but that they ſhould alſo be capable of bringing productive ſeeds, or, in other words, of perpetuating the ſpecies. That I might learn whether the ſeeds of my three gourds enjoyed this prerogative, I cauſed ſome of them to be planted in the ſame place in May 1778, and when they were grown to ſome ſize, they were, as in the foregoing experiment, carefully ſtripped of all their male flowers, one female flower only being left upon each individual. Theſe flowers were furniſhed with ſmall gourds, which grew ripe towards the beginning of autumn, and the ſeeds they produced grew juſt as well as the former; nor can I doubt that they would have brought forth fruit juſt as well, if I had choſen to try them.

From

From theſe facts it appears, that the gourd with ſhield-form fruit does not agree with baſil in the circumſtances attending generation. The ſeeds of baſil, to be productive, muſt be impregnated by the pollen, but the ſeeds of this gourd propagate the ſpecies without ſuch help. The things which I have related furniſh the moſt certain and unequivocal proofs of this aſſertion. It is impoſſible to ſuſpect, that the powder of the male flowers, growing upon the ſame plant with the female flowers, had any ſhare in producing this effect, becauſe I deſtroyed them long before it was ripe. Nor can it reaſonably be preſumed, that any pollen was carried by the air to the ſpot from places at a diſtance. I purpoſely choſe Scandiano (XX), as there was not a gourd of this ſpecies any where in the neighbourhood. We muſt therefore of neceſſity conclude, that neither the embryos of this plant, nor its fructification, depend upon the powder of the ſtamina.

XXII. I now come to the other ſpecies of gourd, the ſecond plant, with male and female flowers growing ſeparately on the ſame individual of which I propoſed to treat (XX). One root of this ſpecies was planted late in the ſpring of 1779, in the ſame garden which the two preceding years had produced the gourd with ſhield-form fruit. The preſent plant was in like manner deprived of all its male buds, as ſoon as they began to appear, and a few female flowers only were left for fructification, ſuppoſing it ſhould take place in theſe circumſtances. That this plant might be inſulated, and that no ſuſpicion might ariſe concerning the influence of adventitious

 pollen,

pollen, I took another precaution, which he who is aware of the delicacy requiſite in ſuch experiments will not regard as unneceſſary. My precaution conſiſted in encloſing, in large bottles of glaſs, thoſe branches of which the female flowers were about to open, and ſo to lute the necks with a thick cement, as to prevent the paſſage of the external air, which is thought by many to be the vehicle of the fecundating powder, into the bottles. This artifice eaſed my mind of another apprehenſion. Some botaniſts pretend, that the fecundation of ſeveral plants is effected by means of inſects. According to them, bees and other winged inſects of the ſame kind, by creeping into flowers to ſuck the honey, or extract the wax, are apt to bruſh away the duſt of the ſtamina, and to convey it on their bodies, from flower to flower; in this manner, ſay they, are plants frequently impregnated. The inſtances of artificial fecundation, which will be hereafter mentioned, incline me to admit this ſuppoſition. The diſpoſition of the bottles in the manner above deſcribed, not only precludes all ſuſpicion of pollen being conveyed to the female flowers by the air, but it likewiſe hinders the acceſs of inſects. Two female flowers about to be expanded, were guarded in this manner by bottles with wide bellies; but the bloſſoms, as well as the young gourds belonging to them, came to nothing. The cauſe of this failure was the perſpirable matter iſſuing from the two encloſed branches: it collected at firſt in the form of dew upon the inſide of the bottles, and afterwards ran down in little ſtreams

upon

upon the flowers; hence they periſhed in three days. But the unſucceſsful termination of this trial did not cauſe me to abandon my purſuit; it only taught me how to enſure ſucceſs. I had only, as appeared from the event of this experiment, to prevent the flowers from reſting upon the ſide of the bottle. I therefore contrived to ſuſpend them in the middle, ſo that they might be only juſt as much moiſtened by the perſpirable matter, as would be ſufficient to keep them in a ſtate of vigour. In this manner two female flowers were encloſed for eleven days; that is to ſay, from the time they were about to expand, till they were withered. In the interval the fruit had grown a little. Having thus eaſed my mind of fear, leſt any unperceived pollen ſhould have been conveyed to them at this moſt critical period, I removed the bottles on the twelfth day, and expoſed the fruit to the open air till it grew ripe, which happened about the eighth of September. They both ſeemed perfect; and all the inſide of the ſeeds was occupied by the lobes and plantule. The growth of the ſeeds of the former ſpecies gave me ſome reaſon to hope, that thoſe on which I was employed would produce plants; and ſo it really happened: I planted an hundred, fifty taken from each pumpkin, of which eighty-nine were productive, and conſequently eleven failed: the cauſe of their ſterility was the ſame as in a former experiment (in which ſeventeen ſeeds of the other pumpkin proved unproductive (XXI). The plantule or the lobes laboured under ſome vice of organization; a circumſtance by no means ſurprizing, as it is is obſerved in the ſeeds of the ſame plants, when left to the care of Nature. I planted the ſeeds of the preſent ſpecies in 1780, as I had before ſowed thoſe of the former in 1778; in both caſes the reſults relative to fructification were exactly alike. It was therefore reaſonable to conclude, that fructification in this plant does not, in the ſmalleſt degree, depend on the powder of the ſtamina.

CHAP. IV.

GENERATION OF CERTAIN PLANTS PRODUCING MALE AND FEMALE INDIVIDUALS, ON WHICH THE FECUNDATING DUST WAS PREVENTED FROM EXERTING ITS INFLUENCE, VIZ. CANNABIS SATIVA, HEMP, SPINACIA OLERACEA, SPINACH, MERCURIALIS ANNUA, ANNUAL MERCURY.

XXIII. CANNABIS *Sativa*, hemp. During my reſidence at Scandiano, in the ſummer of 1767, I obſerved two phænomena, which deeply engaged my attention. The firſt was upon a ſtalk of hemp, that ſprung up ſpontaneouſly in my garden. It was ſtrong and tall, and on all ſides ſurrounded by a wood of branches. It produced a great number of ſeeds, which, in all appearance were perfectly formed. I found, by experiment, that they were productive, not-

notwithstanding it was the only plant in the garden which is very large, and no male plants grew within a considerable distance. Prejudiced as I was in favour of the universally received sexual system, I felt some slight suspicions arise in my mind from this observation; I could not refrain from mentioning it as a singular occurrence to several friends, and among others to the celebrated naturalist Campi Somasco. I however at that time thought, that the phænomenon might be explained, from the supposition of pollen conveyed by the wind from the hemp-yards, with which the maquisates of Scandiano abounds—more especially as I was aware of the great plenty of this powder, which rises in a thick cloud, when the plant is shaken. I imagined also, that from the greater abundance of pollen afforded by the male hop (*a*), the favourers of the sexual system, might account for the fructification of female plants discovered in places where there are no males.

XXIV. The second phænomenon which I observed the same year, was the following. The inhabitants of the territory of Reggio and Modena, eradicate all the male individuals from the hemp-yards, about the second or third of August, and leave the females till the end of September. In a season favourable to vegetation, the females continue to grow above a month, and to produce new branches. Upon these branches spring flower-buds, which in time expand and bear seeds. Of such female individuals I saw,

(*a*) Humulus Lupulus. Linn.

for

for the firſt time, great numbers growing on the 8th of September, 1767; that is, thirty-ſix days after the male plants had been pulled, as I was aſſured by a perſon of credit. I fixed my attention chiefly upon the late branches, on which as yet the firſt rudiments of the cloſed flowers ſcarce began to appear: I moreover tied a thread looſely round them, that I might try whether the ſeeds, if any ſhould be produced, would arrive at perfection. This actually happened, as their ſhooting ſoon after I ſowed them in October irrefragably proved. This ſecond phænomenon gave riſe to ſerious reflections, as I could not poſſibly at firſt comprehend how the pollen could have fecundated theſe female flowers, which made their appearance upon the branches ſo long after the removal of the male plants. After a little conſideration, I perceived that the advocates of the ſexes might find ſomewhat to adduce in their defence. Notwithſtanding the want of male flowers, it might be ſaid, the pollen which had been ſhed in ſuch abundance, may float in the air ſurrounding the individuals of the other ſex.

XXV. Eight years afterwards, in 1775, a Memoir appeared in Rozier's Journal on the the fecundation of plants, the tenour of which perfectly agrees with my obſervations on hemp. The ſubſtance of this memoir, as far as it relates to our preſent enquiry, is the following. The author reared a female ſtalk of hemp in a pot at Paris, which produced, at the uſual time, ſeeds of the common ſize, conſiſting of two well nouriſhed lobes with plantule or germ. When the ſeeds were planted,

planted, they all without exception germinated, and grew up in a ſhort time. The plant had not been covered by any thing; it ſtood upon a window level with the ground, at a great diſtance from other plants of the ſame ſpecies. The maker of this experiment does not pretend, that it entirely excludes all ſuſpicion of the fecundating duſt, but only that the acceſs of it is highly improbable; he exhorts naturaliſts to repeat and improve his experiment. The author of the Memoir does not make himſelf known any otherwiſe, than by the initials *M. F. de B.* which ſeveral have ſuſpected to deſign the celebrated Mr. Fougeroux de Bondaroi, of the Academy of Sciences, nephew to the illuſtrious Duhamel. However this may be, it appeared to me when I read the account, that this delicate experiment had not been conducted with ſo much attention and ſuch precautions, as to produce conviction in the mind of the reader (*a*).

XXVI.

(*a*) Having communicated in a letter, written Sept. 18, 1777, to my illuſtrious friend Mr. Bonnet, ſome reſults relative to the fecundation of plants, I received a very polite reply, dated Nov. 29, the ſame year. Among other things he mentioned this Memoir, ſuppoſing I had never ſeen it. It was his opinion, that the obſervation of the French Naturaliſt, whom he preſumes to be Mr. Fougeroux, is not ſufficiently deciſive to exclude the fecundating powder. He was induced, by his reſpect to truth, to make ſome ſtrictures on different paſſages of this Memoir; which, as they are connected with my obſervations on generation, it will be proper to tranſcribe. "Various inadvertencies and miſtakes have been committed by M. F. which might be overpaſſed in an author of inferior reputation. I ſhall point them out from a ſhort

XXVI. In relating my ſubſequent experiments on the generation of plants, I ſhall give

ſhort account I gave to another correſpondent. The propoſitions diſtinguiſhed by Italicks are the words of M. F.

" *We are not ſure, that the chicken exiſts before impregnation in the egg, and we muſt conſequently be ignorant, whether the plant exiſts in the ſeed before fecundation.*

" The learned naturaliſt ought, I think, to have expreſſed himſelf very differently after the fine diſcovery of Haller, of the pre-exiſtence of the chicken, after your's of the pre-exiſtence of the tadpole, and Mr. Muller's and my obſervations on the pods of peaſe. *Palingen*, T. 1. p. 416. In my eſſay on the fecundation of plants, printed in Rozier's Journal, for Oct. 1774, I had brought to the reader's recollection theſe ſeveral diſcoveries on vegetables and animals, as ſtrongly tending to eſtabliſh the great probability of the pre-exiſtence of the germ; and M. F.'s Memoir evidently refers to mine.

" *But is fecundation, by means of pollen, neceſſary in all plants? Have none their parts of generation concealed, like the lice of plants (pucerons)?*

" The parts of generation in the lice of plants are not *concealed*. On the contrary they are quite obvious. I dwell pretty largely on the amours of theſe inſects in my *Inſectology* publiſhed in 1745. I there deſcribe at length their genital organs.

" *Laying aſide all ſyſtematical ideas, I abide by facts and obſervations, and follow a celebrated guide, M. Duhamel; who, before me, has multiplied facts and obſervations, without venturing to decide.*

" It is doubtleſs commendable to abide in phyſics by *facts* and *obſervations*. But ſound logic more than barely allows us to deduce the moſt immediate and direct conſequences, as I have endeavoured to do in my works, and in the eſſay which Mr. F. ſeems to have held in view. He expreſſes his intention to take Mr. Duhamel for his guide. But the fine diſcoveries of Haller, Spallanzani, &c. were unknown to Mr. Duhamel when he compoſed his excellent Natural Hiſtory of Trees, which was publiſhed in 1758. Mr. F. who publiſhed his Memoir in

1775,

give an account of the means employed to prevent the accefs of the pollen. Six fmall ftalks

1775, was naturally called upon to weigh the confequences refulting from thefe difcoveries, and to avail himfelf of the analogical deductions drawn by me refpecting vegetables. This, however, he has not done, and I cannot but be furprized at it.

" *Thofe who fuppofe the plant to exift in the feed before fecundation, confider the pollen as confifting of a number of fheaths and cafes, of which each contains a number of feeds floating in a thin liquor.*

" Some naturalifts, as Needham, have confidered the powder of the ftamina in this light, but they do not allow *the plants exiftence in the feed before fecundation.* Mr. Needham, who fo well explained the compofition of the pollen, believed the germ to be in the pollen itfelf. Mr. F. has therefore expreffed himfelf very incorrectly; but when he immediately afterwards fubjoins, *the plant, according to this opinion, exifts before fecundation in the powder of the ftamina*, he falls into the moft glaring contradiction. *That the plant exifts in the feeds before fecundation*, is an hypothefis which excludes the opinion, *that the plant exifts before fecundation in the powder of the ftamina.*

" *But here only human intellect is bewildered. How can we, ufing our reafon, imagine the germs of all plants to be inclofed in fuch a germ. What an abyfs! Let us quit this clue as likely to miflead us.*

" What meaning has the word *only* in this place: It is precifely by the ufe of our reafon which does not imagine but *conceive*, that we perfuade ourfelves of the probability of that hypothefis, according to which germs are inclofed one within another. If M. F. had read my Memoir more attentively, and meditated longer on the fubject, his mode of expreffion would have been more philofophical. Does it then belong to the imagination to decide concerning matters lying exclufively within the province of reafon? Can the imagination reprefent to itfelf an animalcule many million of times fmaller than a *mite?* Can it reprefent to itfelf a globule of light, feveral thoufands of which fall at once on the eye of fuch an animal? No;

ſtalks of hemp were tranſplanted from an hemp-yard into ſix pots. As ſoon as they had

No; never will reaſoning or calculation overturn the hypotheſis of *involution*. Great and ſmall are merely relative terms; and we are acquainted with ſurprizing facts that lead us to this hypotheſis.

" *According to Haller, irritability is the principle which conſtitutes animality and produces life. The powder of the ſtamina exciting the irritability, and occaſioning the fluids to be impelled in the organized body, produces in the vegetable the ſame effects as the ſpermatic liquor in the animal germ.*

" Haller did not admit irritability in vegetables. He could not therefore attribute to the powder of the ſtamina that *ſtimulating* property which he acknowledges in the ſpermatic liquor of the animal: and yet, from M. F.'s expreſſion, it would be ſuppoſed that he does. But it was I, who, in my eſſay *on the fecundation of plants*, ſought to apply irritability to the fecundation of vegetables—not, however, till I had declared my ignorance of any fact which rigorouſly proves the exiſtence of irritability in the vegetable kingdom. Upon this occaſion, I threw out ſome logical reflections, calculated to cauſe the ſuſpenſion of the reader's judgment. Let me be allowed to obſerve, that no author in natural hiſtory has been more careful to diſtinguiſh conjectures from facts: but I do not believe that conjectures are unwarrantable. I have therefore confined myſelf to ſhew how we may conceive the influence of irritability in vegetables, ſuppoſing them to poſſeſs this property. M. F. read me too rapidly, and without reflecting ſufficiently upon my mode of arguing. Moreover, the irritability of plants which I did but conjecture to exiſt, ſeems to have been well obſerved by the celebrated Gmelin in ſome ſpecies; but I was unacquainted with his obſervations in September 1774, when I was compoſing my eſſay. That author, in his Diſſertation on Irritability, ſhews, that the antheræ, or ſummits of the ſtamina, appeared to him *irritable, or at leaſt endowed with a property nearly approaching to the property of animals called irritability*. He adduces many inſtances exceedingly remarkable, and, among others, that of the orchis. *The antheræ* had attained a certain height, and the male plants could be diſtinguiſhed from the females, the former, which were four in number, were eradicated, and the two females only were preſerved. About twenty days before they began to flower they were carried into a room looking towards the ſouth, where they ſtood forty-two days. There were two windows in the room, which were always kept ſhut, as alſo was the door, except when

antheræ of this plant, he obſerves, *recently gathered, and irritated in a warm place, appeared to contract, and then to relax, and to undergo a kind of tremulation.* He adds, that he has frequently made the experiment, and that *it always ſucceeded.*

"*According to Mr. Bonnet, the ſeminal fluid, in producing impregnation, effects only the evolution of what was already formed.*

"This certainly is my opinion: but to ſay ſo much is ſaying nothing; for ſuch vague expreſſions do not ſerve to diſtinguiſh my notions from thoſe of other naturaliſts who reject the doctrine of Epigeneſis. I have reaſon to preſume, that the worthy M. F. has not ſtudied with any care the nature and the connection of my principles on generation. He was doubtleſs too much prejudiced with the idea of generation being a myſtery not yet penetrated by the ableſt naturaliſts. God forbid that I ſhould have the preſumption to think that I have drawn aſide the curtain that covers the myſtery: I have only attempted to lift up a corner of this curtain.

"*When we read the opinions of moſt naturaliſts, do we not incline to believe, that each, out of affection to his own ideas, flatters himſelf that he has divined the ſecret of Nature?*

"I muſt confeſs that there are phyſiologiſts and natural hiſtorians who deſerve this reproach; but there are others who do not deſerve it; and M. F. ought to have excepted them. You have read me, and, what is more, have ſtudied me. You, therefore, know whether I have ever propoſed my notions as a *man, who flattered himſelf that he had divined the ſecret of Nature.*"

I went in to water the plants. They thrived, notwithſtanding their confinement, on account of the rays of the ſun ſhining through the windows upon them ſeveral hours a day. Although this precaution ſeemed preferable to that of the French anonymous writer, who had placed his female plant upon a window, it did not ſatisfy me. The pollen indeed ſuppoſed to be diſſeminated through the air at the flowering of the hemp-yards, could not very readily find its way into an apartment kept ſo cloſely ſhut, yet it cannot be aſſerted that its ingreſs was totally prevented; and yet it was of the utmoſt importance to hinder it, at leaſt, from coming in contact with the flowers. To attain this end effectually, when I brought the pots into the room I immediately introduced two branches of my two plants, upon which ſome buds were growing, into two glaſs bottles, with long necks and large bodies. The necks were perfectly cloſed with maſtic, without injuring the branches. By being perfectly cloſed I mean, that all communication between the external air and the inſide of the bottles was cut off, as was evident from the following deciſive proof. Along with the branch of hemp, I inſerted, for about five inches, an open tube of glaſs, which was imbedded in the maſtic; the other end projected four feet from the neck of each bottle, and was immerſed in a baſon of water to the depth of ſeveral inches; but I had firſt taken care to ſuck a portion of air out of the bottles. The water aſcended to the height of a foot and three quarters into the tube, at which it conſtantly ſtood, being only ſubject

to

to ſuch variations as are obſerved in barometers, for the tube was now actually changed into one of thoſe inſtruments. The permanent elevation of the water in the tube was a certain ſign, that no external air found its way into the bottles: had a ſtream of air made its way through the maſtic, or any part of the bottle, it is obvious, that the water in the tube muſt have deſcended to the level of the baſon. I muſt own, indeed, that I was obliged to renew the experiment ſeveral times before I ſucceeded. With this apparatus I was certain that the external air, and conſequently that the pollen, which may be ſuppoſed to be diſperſed through it at the approaching flowering of the hemp-yards, could find no acceſs to the confined branches.

XXVII. But all theſe precautions ſeemed inſufficient to render the experiment quite unexceptionable. It was neceſſary, not only to remove all ſuſpicion of external pollen getting into the bottles, but likewiſe to watch whether any of this powder was produced within. The botanical reader immediately comprehends the reaſon of my apprehenſion. It has been obſerved by Linnæus, Haller, Duhamel, and others, that male flowers are not very infrequently found upon female individuals: a root of ſpinach, of which I ſhall ſpeak below, furniſhed me with a remarkable inſtance; and the ſpecies in queſtion is ſubject to the ſame accident, as I was informed by Mr. Bonnet, in a letter dated Auguſt 15, 1778. The letter gave me notice of an experiment which he undertook upon hemp, after I had communicated mine to him. The paragraph to which I allude is the following: " I began this year ſome experiments upon hemp. I have followed the method which I employed for rearing the lice of plants in ſolitude. My plants were covered with large tubes of glaſs, hermetically ſealed at the top, and with the bottom ſunk in the earth. But fortune did favour me—inſtead of a female I had a male plant in one inſtance, and in another a plant of great expectation, after putting forth many flowers with piſtils, produced ſome with ſtamina, cloſe to the former, which totally diſconcerted the experiment." It therefore became me to watch diligently, whether any male flower was produced among the females; I therefore carefully examined twice a day, both with my naked eye and a glaſs, the confined branches, which were as open to inſpection, as if they had been growing in the open air. In the mean time, I did not neglect the other parts of my two plants. That the examination might be more certain, I ſtripped off all the branches except thoſe which were inſerted into the glaſſes, and took care to inſpect the ſtems as cloſely as the branches. But I can aſſure the reader, with the utmoſt confidence, that the flowers which ſucceſſively made their appearance, both on the bare ſtems and branches, were all female without a ſingle exception. For the ſake of perſpicuity, I will firſt deſcribe the trunks, and then the branches. The former began to put forth their flowers towards the end of Auguſt, at which time the hemp-yards were alſo in bloom. Notwithſtanding their conſtant con-

confinement, they produced ſeeds, and ripened them by the end of September. When I compared theſe ſeeds with ſome which I gathered in the hemp-yards, I found that they were ſmaller. Beſides the number of ſeeds produced by the ſtems in the hemp-yards, were in like circumſtances conſiderably more numerous, than by the two ſtalks confined in my apartment. But notwithſtanding theſe differences, the ſeeds grew nearly as well in the apartment as thoſe which grew in the open air, as I found by ſowing ſeveral hundreds of both ſorts. Such are the reſults afforded by the two ſtems.

XXVIII. I am now to relate what happened to the two branches. The flowers, which nearly kept pace with thoſe of the trunks, were ſucceeded by ſeeds; towards the 20th of September they had acquired ſuch a ſize, as inclined me to think them ripe; I therefore reſolved to take the branches out of the bottles, and I found in fact, that more than an hundred were arrived at maturity; this was doubtleſs in ſome meaſure owing to their having received no moleſtation from the perſpirable matter, which was collected in large quantities in the belly of the bottles. By diſſecting ſome of the ripe ſeeds, and others that were more or leſs diſtant from this ſtate, I was enabled to make the following obſervations: When the parts ſurrounding the leaſt forward are removed, a greeniſh pyriform body preſents itſelf with two points like ſmall antennæ, which are the rudiments of the piſtils. In the pyriform body is contained a ſolid nucleus, ſemi-gelatinous, and conſiſting of an homogeneous ſubſtance. The nucleus in ſeeds further advanced, exhibits an excavation in the center, full of a liquid which, in ſeeds yet nearer maturity, is thicker; and in this inſpiſſated matter may be ſeen a white point, which is eaſily known to be the lobes and plantule. If ſeeds ſtill more advanced be analyſed, in opening of the lobes, which are bent into the ſhape of an arch, a cavity or depreſſion is obſerved, and in it are lodged two whitiſh dentated leaves, which are inſerted in the plantule, or rather ariſe from it. Theſe leaflets are larger in ripe ſeeds. Such were the appearances of the product of the branches; and ſeeds gathered in the hemp-yards furniſhed me with the ſame obſervations. Hence I was obliged to infer, that Nature, in the production, or to ſpeak more properly, in the evolution of the ſeeds, had operated alike in the cloſed bottles, and in the open fields.

I ſaid before, that the ſeeds of the two confined ſtems were ſmaller and in leſs number, than of thoſe in the hemp-yards. I may now add, that the ſmall ſize of the ſeeds that grew in the bottles was ſtill more remarkable, and that equal branches uſually produce twice the number. But I ſhall be aſked, did ſeeds ſo much ſmaller and leſs numerous grow when they were ſown. To this curious and important point all my enquiries tended. Of a hundred and ſixteen ſeeds, which were all that ſeemed to be ripe, one half, viz. fifty-eight, were ſet in a pot, and they all, except five, came up.

XXIX. The reſult of another experiment correſponded perfectly with the foregoing.

Though a large quantity of pollen arifes from the hemp-yards into the atmofphere, at the feafon of flowering, and is tranfported to no inconfiderable diftance, it is reafonable to fuppofe, that in the courfe of feveral months it muft be loft; for the rain and fnow cannot fail to carry it down to the ground, and free the air from it: fhould any continue to float in the atmofphere, it will probably in time lofe its prolific virtue. This fuppofition, which feems well founded, as will be more particularly fhewn in the fucceeding chapter, induced me to make fome female plants of hemp blow before the hemp-yards were in flower. With this view, I fet at Pavia twelve hemp-feeds in twelve pots, before the middle of November; the feeds were fome that remained of the product of the confined branches (XXVIII). Although the feafon was unfavourable to vegetation, eleven out of the twelve germinated, and continued to grow, though but flowly, the whole winter, the pots having been removed into a warm room. When fpring arrived, the windows were thrown open, and left fo till I had occafion for the plants. Meanwhile they continued to advance, and about the 20th of May, the male and female individuals could be diftinguifhed. The former were deftroyed. The remaining females, four in number, were expofed to the air and fun on the outfide of a window. That I might examine with greater eafe and certainty, whether any male bloffoms were produced upon thefe female plants, I cut away all the branches, and preferved only the ftalks, as in the experiment related in the XXVIIth

 paragraph.

paragraph. But fortunately no flowers of this ſort ever appeared. Towards the end of May, flowers with piſtils began to appear, ſix weeks before the flowering of the hemp-yards beyond the Po (for there is no hemp raiſed in the neighbourhood of Pavia) and conſequently before the diſſemination of the freſh pollen. About the 8th of June moſt of the ſeeds were ripe. They were quite as thick and large as upon the ſtems reared in the fields; I ſowed ſeveral hundreds, and moſt of them germinated. One immediate conſequence may be deduced from theſe variations of my experiment. The perfect fructification of hemp is entirely independent of the action of the fecundating duſt.

XXX. But enough of the experiments on hemp. I proceed to ſpeak, but much more briefly, of ſpinach (*Spinacia Oleracea*). I made ſeveral different trials with this plant in the courſe of ſome years.

In the firſt place, I tranſplanted in May ſeveral roots into my garden at Pavia. As ſoon as the different individuals could be diſtinguiſhed, I deſtroyed the males. The females, although they remained alone in the garden, produced plenty of ſeeds, which, when ſown, germinated in great abundance. At the time of flowering, I watched with great care, whether any boſſoms with ſtamina opened: my illuſtrious colleague and friend Counſellor Scopoli, alſo viſited my ſpinaches; but neither could that expert botaniſt or myſelf perceive any.

I next covered a plant of ſpinach with the receiver of an air-pump; the lower end was preſſed to ſome depth into the earth of the

pot.

pot. Thus the communication between the external and the internal air was cut off, unleſs the former may be ſuppoſed to have found its way into the receiver, through pores in the mould. I made uſe of this apparatus a few days before the opening of the flowers; but was obliged, in thirteen days, to remove the receiver, for the leaves began to wither, probably on account of their confinement. Several flowers with piſtils had in the mean time ſet, and the appearance of the ſeeds promiſed that they would do well; nor was I deceived in my expectation, for they ripened perfectly, and when ſown, came up in great abundance.

XXXI. In the Modaneſe and in the Marquiſate of Scandiano, the ſpinach raiſed by the people of the country, is uſually in flower from the 20th of May to the middle of June, ſo that the ſeeds are commonly gathered at the beginning of July. I took advantage of this practice to procure piſtilliferous flowers, a long time after the bloſſoms with ſtamina had decayed. For this purpoſe, I ſowed ſome ſpinach towards the end of May; the female plants produced flowers with piſtils in September, and many of the ſeeds came to maturity; but many others were checked by the beginning coldneſs of the ſeaſon: but thoſe which grew ripe were ſown, and they germinated. Notwithſtanding, therefore, near ſix weeks had elapſed ſince the falling of the ſtaminiferous flowers in the fields and gardens, my ſeeds were productive. It is difficult to imagine, that the pollen could have been the cauſe of this fecundity; and the rather, ſince in ſpinach it is far from being

being in ſuch abundance, as to be capable of diſſemination, like that of hemp, far and wide through the air. My ſpinaches were, moreover, conſtantly kept in an apartment.

XXXII. I nevertheleſs undertook another trial, which I believed would be deciſive. I had ſeveral young ſpinaches tranſplanted into pots in Auguſt at Pavia, one into each pot. Great attention being paid to them, they grew before winter to a conſiderable height. And being kept in a warm room, they made ſome further progreſs during the courſe of this ſeaſon; and before the end of March, the female plants, which were three in number, began to put forth flower-buds with piſtils. But here an accident, which is not very uncommon, and to which I have already adverted (XXVII), occurred. In one of my daily viſits to my three plants, I perceived upon one individual an unexpected conjunction of male and female flowers, growing cloſe together, and forming very elegant groups.

The bloſſoms with piſtils were very conſpicuous, but thoſe with ſtamina were ſo little advanced, that they could not be diſtinguiſhed by the naked eye. Both ſorts appeared to be equally numerous, but the union extended only to two branches—all the reſt bore female bloſſoms only. I may here incidentally remark, that the great abundance of the male flowers, in the preſent caſe, is a very ſingular phænomenon. I have read in botanical writers, that a few male flowers are ſometimes found in company with females, but never that they amount to an equal number, a circumſtance that excited my my admiration with reſpect to this individual; for I counted two hundred and ſeventy-five male buds. This unforeſeen accident determined me to extirpate the plant. I conſider it as very fortunate, that I obſerved it in time, while the male bloſſoms were yet in a very immature ſtate, and conſequently before the pollen could yet have produced any effect. Having thus freed myſelf from the trouble of watching this extraordinary individual, I continued to obſerve the two others; they produced only bloſſoms with piſtils. April was the ſeaſon of the maturity of theſe bloſſoms, which is generally eſtimated by their aptitude to be fecundated: it was earlier by thirty-five days, than the uſual flowering of this plant in the environs of Pavia. Before the end of May, the ſeeds of my two plants were almoſt all ripe. Every branch and every twig was loaded, and they were of equal ſize with the ſeeds produced in the neighbouring gardens the preceding year. To complete the experiment, it remained to try whether they would germinate. Of a hundred and fifty, choſen among the whole product as the fineſt, a hundred and thirty-two came up; and of a hundred, which I afterwards ſowed, ninety-three grew. Theſe proofs obliged me to conclude, that the ſeeds, notwithſtanding the privation of the pollen, were productive.

XXXIII. The next and laſt plant producing male and female individuals, which I mentioned, is annual mercury (*mercurialis annua*). Five very ſmall plants were removed from a garden, on the 22d of Auguſt, into five pots. They were managed in the

ſame

ſame manner as the ſpinach during the winter (XXXII), and were all ſo far advanced at the beginning of ſpring, that there was no difficulty in diſtinguiſhing the males from the females; of the latter there were three, and theſe alone were preſerved. By the 24th of March bloſſoms with piſtils appeared upon ſeveral branches, growing out of the axillæ of the leaves, and in a few days more the number was exceedingly increaſed. They were borne upon ſhort flower-ſtalks, and, as uſual, conſiſted of two ſmall ſeeds reſembling teſticles. They were of a green colour and hairy. But here the event was juſt contrary to what happened in hemp and ſpinach. The greater part of the bloſſoms dropped prematurely; of the few that remained the ſeeds grew for ſome time, but fell before they were ripe, and when ſown, they did not ſpring. As this took place before the male plants in the gardens and the fields about Pavia were in flower, I began to ſuppoſe mercury to be one of thoſe numerous vegetables, which cannot propagate the ſpecies without the powder of the ſtamina. Meanwhile my three plants continued to put forth new branches, and the old ones, inſtead of withering, vegetated with great vigour; but ſtill the ſeeds dropped prematurely. This gradual evolution and production of freſh branches, was of ſuch long continuance, that they ſhewed no tendency to decay, but were producing bloſſoms with piſtils when the mercury in the fields was in flower. I therefore began to entertain hopes, that the ſeeds now put forth, and thoſe which ſhould follow, would ſucceed better than the earlier feeds, more efpecially as the pots were expofed to the open air upon a window, and looked into a garden, in which grew feveral male individuals of this fpecies. But my expectations were difappointed: as long as the three plants continued to thrive, the feeds dropped almoft as foon as they appeared; nor did one of thofe that were fown ever come up. I repeated the experiment two fucceeding years with the fame event.

XXXIV. It therefore became neceffary to vary the mode of conducting it. Being more confirmed in my fufpicion, that the fterility arofe from want of pollen, which, though it was at no great diftance, did not reach my plants, I determined to bring it nearer; without, however, fetting individuals of the different fexes in the fame place. Two male plants of mercury, reared the next year in two pots, were placed on the outfide of a window, and two females growing likewife in pots, were fet on the outfide of another window. Both windows belonged to one room, and had the fame afpect. The four roots of mercury were nearly of the fame age, and of the fame fize. And I waited with great anxiety to fee whether the females, on account of their vicinity, would be impregnated by the males. The feeds were conftantly falling, but not in fuch abundance as in the former experiment, when the males were at a much greater diftance (XXXIII). Thofe which adhered went on thriving, and feemed as if they would ripen; and they did accordingly arrive at maturity, and, what is of more confequence, were productive; for

foon

ſoon after I had ſown them in a pot, I had the pleaſure of ſeeing them ſpring. It therefore appeared probable, that the vicinity of the males to the females had been inſtrumental in occaſioning fecundation: their influence could ſcarce be derived from any ſource, beſides the action of the contiguous pollen.

XXXV. This experiment obviouſly required another: it was proper to bring the different individuals nearer to each other; I accordingly placed two males and two females upon the ſame window. It now became manifeſt, how much influence the approximation of the two ſexes has upon fecundation. The two females retained almoſt all the ſeeds which were produced at this time, exceeding an hundred. The ſeeds grew perfectly ripe, and when put into the ground, were unfolded into as many plants.

XXXVI. The females having ſeveral branches in vigour, and ſtill continuing to vegetate, an experiment, the reverſe of the two preceding, ſuggeſted itſelf to me. I removed the pots in which the male plants grew into another chamber. This ſeparation ſerved further to corroborate the neceſſity of the pollen in mercury; for in conſequence of its entire removal, all the ſeeds afterwards produced, by the growing branches fell before their time, and were unproductive.

Moreover, it is by no means ſurprizing that females, at a diſtance from males, ſhould be ſterile. The pollen is in too ſmall a quantity to be widely diſſeminated. The inconſiderable quantity which was afforded by ſeveral

veral male plants, at a time when the antheræ, viewed by the microſcope, appeared full, gave me ample proof of its ſcarcity.

I ſhall make upon mercury another obſervation, very neceſſary to be known. I have informed the reader, that moſt of the unimpregnated ſeeds wither and drop, while a few grow to ſome ſize, but are not productive (XXXIII). The ſcope of my reſearches required that I ſhould examine, whether theſe ſeeds conſiſt of lobes and a plantule. By analyſing them repeatedly, I found that the ſeeds, which fall ſoon after the flowering, are full of an homogeneous and gelatinous ſubſtance, and afford no veſtige of plantule or lobes. But impregnated ſeeds, examined at the ſame period, that is to ſay, a little after the ſeaſon of flowering, exhibited the ſame appearances. Many days elapſed before the lobes and the plantule could be diſcerned, an obſervation that is not confined to mercury, but extends to every plant hitherto examined by me. In thoſe ſeeds which made a tolerable progreſs in their growth before they fell, appearances were very different. They firſt became full of an almoſt fluid whitiſh matter, which was gradually inſpiſſated; and in the midſt of this matter, after ſome time, might be ſeen a little body, ſo ſoft as to adhere to the finger when it was touched; but it afterwards made ſome reſiſtance to the touch, and now evidently appeared to be the lobes and plantule; which parts grew more conſpicuous and diſtinct as the ſeeds advanced. The plantule had the ſhape of a cone, dilated to-

wards

wards the baſe into two flat circular bodies, which were the lobes. The ſame was alſo the figure of the lobes and plantule contained in productive ſeeds.

CHAP. V.

RECAPITULATION OF THE PRINCIPAL CONSEQUENCES OF THE FACTS RELATED IN THE FOUR PRECEDING CHAPTERS.

REFLECTIONS.

XXXVII. ALL theſe facts convey, in my opinion, inſtruction ſufficient to ſerve as a foundation for ſpeculating on the mode obſerved by Nature in the generation of plants. If we inſpect the ovaria before fecundation, we find the ſeeds externally ſolid: they continue in this ſtate for ſome time after fecundation. They afterwards became hollow within; in the cavity appears a ſmall body, not diſtinguiſhable at firſt, but afterwards known to be the embryo or the plantule with the lobes. The embryo ſometimes ſeems detached from the ſeeds, and floating in a liquid, and ſometimes is attached to them by one or more connecting threads (II, III, IV, V, VI, VII, VIII, IX, X, XI, XII, XIII. XXXVI).

As

As theſe facts ſeemed favourable to the opinion of thoſe, who ſuppoſe the embryos of plants to pre-exiſt in the powder of the antheræ, and thence to paſs into the ſeeds, which therefore do not contain an embryo before fecundation, I was led to turn my reſearches towards the powder. I examined it with the microſcope, but could not by any means find that the embryos, or any beings analogous to them, are lodged in it (XV). Hence I proceeded to the expedient of interrogating Nature in another manner: I endeavoured to prevent the acceſs of the impregnating duſt to the ovaria, ſuppoſing that ſuch means would furniſh the clue to this abſtruſe myſtery. Three deſcriptions of plants were ſubmitted to experiment; hermaphrodites; thoſe which bear ſeparately upon the ſame individual, male and female flowers; and, laſtly, thoſe which produce both male and female individuals. The antheræ of the firſt were cut away before the ripening of the pollen; in the ſecond the male flowers were extirpated, and the female individuals of the third were inſulated. In the hermaphrodite plants it appeared, that the privation of the pollen does not prevent the appearance of the embryo in the ſeeds, though the ſeeds are incapable of germinating; in ſome plants the abſence of the pollen neither hinders the embryos from appearing, nor the ſeeds from being productive; the ſame obſervation is to be applied to ſeveral plants with female individuals; but in other plants of the ſame claſs, the want of pollen renders the ſeeds ſterile, though the embryo appears in them,

 (XVIII,

(XVIII, XIX, XX, XXI, XXII. XXVI, XXVII, XXVIII, XXIX, XXX, XXXI, XXXII, XXXIII, XXXIV, XXXV, XXXVI).

The direct and immediate consequence of these results is obvious: for if the embryo appears without the co-operation of the pollen, and if besides, in many cases, the seeds grow, it is clear that their existence can have no dependence on the pollen; and upon those occasions on which the seeds do not grow for want of it, we can only say, that they stand in need of the condition requisite for their further evolution, as the fetuses of animals do not thrive for want of seed, although they pre-exist in the ovarium of the female. Another consequence not less evident is, that as the embryos do not belong to the pollen, they must necessarily appertain to the ovarium.

Some naturalists, of no vulgar reputation, have supposed, that the embryos of plants are formed by two principles, one contributed by the powder of the stamina, the other by the pistils. But my observations, which prove that the embryo has no dependence upon the pollen, clearly prove the falsehood of this supposition.

XXXVIII. But the embryo, which I have shewn to belong exclusively to the ovarium, may be conceived to belong to it in two ways. It may be formed of matter furnished by the parent plant, or it may pre-exist. Now, which of these ways are we to admit? An Epigenesist, a Count Buffon, accustomed to view Nature under an angle proportional to his favourite ideas, would not probably hesitate,

tate, if the facts obſerved by me had paſſed under his inſpection, to embrace the former opinion. The embryo, not being at firſt viſible in the cavity of the ſeeds, but appearing afterwards in the form of a gelatinous point, without any determinate ſhape, floating beſides, in a liquid without any perceptible connection with the ſeeds, continuing to grow and becoming organized in it—Theſe arguments would no doubt be ſufficient to lead that famous naturaliſt into the belief of the embryo's formation in the ſeed; he would perhaps alſo believe, that it is formed of the liquor in which it floats. But if we lay aſide all prejudice in favour of any ſect, and apply the ſtrict principles of logic, we ſhall find theſe arguments inadequate. In the firſt place, that a liquor, or any other inorganic ſubſtance, ſhould nouriſh and promote the growth of an organized body, is a propoſition eaſily conceived, and is indeed proved, by daily obſervations on ourſelves, on animals, and plants. But it is not ſo eaſy to conceive, that an inorganic and ſhapeleſs body, whether liquid or ſolid, can be organized by mechanical laws only: and with all his eloquence, and by all imaginable efforts, the naturaliſt above-mentioned has never ſhewn, that one animal, or one vegetable, even among thoſe that appear the leaſt perfect, and the leaſt organized, owes its origin to a mechanical appoſition of parts. Secondly, though the embryo does not appear to be organized, this is by no means a ſure proof that it is not. During the firſt hours of incubation, the organization of the chicken is not perceptible; and yet we cannot doubt that it is orgazined, if it can be proved to exiſt before fecundation. Now Haller has given the proof of this with reſpect to the chicken; and I have given it with reſpect to newts, frogs, and toads. It is therefore beyond all doubt, that theſe amphibious animals are organized before fecundation. But their organization is not then apparent; mere appearances would lead us to ſuppoſe them formleſs bodies; for we ſee nothing but ſpherules, conſiſting of a cover or ſkin filled with a ſemifluid matter. The ſame ſeemingly inorganic ſtructure continues for ſome time after fecundation. The embryos of plants, then, may alſo be organized, although they do not appear to be ſo; and I have certain proofs that this organization, though not at firſt apparent, is actually preſent. When I opened the ſeeds at the time the gelatinous point began to appear in the midſt of the liquid, and examined it without uſing any method of preparation, I could diſcover nothing that had the appearance of being organized; I ſaw neither lobes nor plantule, nothing in ſhort but a mucilaginous particle. But if I firſt boiled the ſeeds, this point oftener than once put on a different aſpect. It was not only no longer gelatinous, but when I turned it about, and preſſed it gently with the point of a needle, it would ſometimes ſplit into two exceedingly minute pieces, in the middle of which was implanted a pointed atom: it was not very difficult to diſcover that theſe parts were the lobes and plantule, as was ſtill more manifeſt when a microſcope of great magnifying power was employed. This point, therefore, proved to be organized at a time when it was not ſuſpected;

pected; nor is this the firſt occaſion, on which the action of fire has been uſed as a convenient method of aſcertaining the organization of a body which had no ſuch appearance. It is well known to the cultivators of Natural Hiſtory, that the worms generated by thoſe winged inſects, uſually called *carrion flies*, after having acquired, by feeding on putrefied fleſh, a proper ſize, are formed into an elongated bowl, in which the fly is gradually elaborated; or, to ſpeak more properly, unfolded. If theſe bowls be opened a few days after they are formed, no lineaments of a fly preſent themſelves to the ſight; we find only a kind of pap, without any ſign of organization. But the ſcene changes, if the bowls be boiled at different periods; on the third day the legs are viſible, on the fourth the wings, ſoon afterwards the proboſcis, and laſtly the animal perfectly formed. Theſe obſervations were firſt made by Reaumur, and in years paſt I have amuſed myſelf with repeating them.

But the application of heat made me acquainted with another truth. By boiling I diſcovered the point, or the embryo, ſome days before it uſually appears. I have not ſeen it indeed in ſeeds, containing a liquor in which the embryo floats, but in ſome of thoſe in which the liquor coagulates and is inſpiſſated. Several ſeeds were taken from the ſame ovarium, and ſome of them were boiled for a few inſtants, and the reſt left to themſelves. In the latter I could not diſcern the embryo, but it was plainly to be diſtinguiſhed in the former. The difference ariſes from the conſolidation of the particle, or the

 embryo,

embryo, by the boiling water; in the other ſeeds it did not ſtrike the ſight, on account of its tranſparency and fluidity.

XXXIX. I come to the objection derived from the inſulation of the embryo in the ſeeds; from this circumſtance it might be ſuppoſed to have no dependence upon them, and conſequently to originate from the liquor in which it floats. The embryo certainly, in ſome caſes, ſeemed to ſwim at liberty in the liquor, nor could I diſcover any connection with the ſeeds, but on other occaſions I manifeſtly diſcovered the connecting medium, as in the common bean, the radiſh, and the Syrian ſhrubby mallow; it conſiſts of a mucilaginous filament, of which one end inſinuates itſelf between the lobes, and is inſerted into the plantule, and the other end paſſes out of the lobes, and is attached to the inſide of the ſeed (VI. VIII. XIX). This filament probably conſiſts of a number of veſſels, deſigned, like the umbilical cord in animals, to convey nutriment to the plantule and lobes, whether the nutriment be the liquor lying in the cavity of the ſeeds, filtered and rendered perfect in theſe veſſels, or the ſubſtance itſelf of the ſeeds, at the expence of which the lobes and plantule grow; for it conſtantly happens, that as theſe parts are enlarged, the internal ſubſtance of the ſeed is deſtroyed, and at length reduced to a mere ſkin. But beſides the manifeſt communication formed by this filament between the embryo and the ſeeds, I have found, in more inſtances than one, that it communicates at other parts; as is obvious in broom, peaſe, kidney beans, chick-peaſe (IV. VII. IX).

If

If I have ſometimes not been able to find any ligament paſſing from the embryo to the ſeed, it is not therefore to be concluded, that there exiſts none, but that it cannot be diſcerned, either on account of its minuteneſs or tranſparency. For as the embryos, in which the filament is viſible, are unfolded within the cavity of the ſeeds, in the ſame invariable manner as is obſerved by Nature, in embryos appearing without this connecting medium, we have juſt reaſon to infer, that it exiſts in the latter as much as in the former. That the minuteneſs, and much more the tranſparency of theſe cords, may hinder them from being ſeen; not to mention the proof afforded by the embryos themſelves before they are ſubjected to the action of fire (XXXVIII), is confirmed, by a ſtriking inſtance, in the tadpoles of the fetid toad, while they as yet float in the liquor of the amnion: theſe tadpoles are provided with an umbilical cord, but which is not viſible on account of its tranſparency.

As it is evident, therefore, that we have no foundation for ſuppoſing the embryo to be mechanically formed in the ovarium of the plant, and as it has been moreover proved, that it does not depend upon the powder of the male, it remains to be inferred, that it pre-exiſts in the ovarium. In favour of its pre-exiſtence we have another very powerful argument, in the communication effected by the filaments between the embryo and the ſeeds; for hence it is clear, that the ſeed and the embryo compoſe but one organic whole. As then from all my obſervations it appears, that the ſeeds pre-exiſt in the ovarium long before the opening of the flowers, I ſee no reaſon why we ſhould not aſſert the ſame thing of the embryos.

XL. The celebrated Muller, a gentleman of Denmark, made at Geneva, ſeveral years ago, under the inſpection of Mr. Bonnet, an obſervation, nearly approaching to thoſe related in this Diſſertation. He found the ſeeds of peaſe arranged in rows within the pods before the opening of the flowers, and conſequently before the action of the pollen had been exerted; but he was not ſolicitous to puſh his obſervation any farther, by examining the inſide of the ſeeds, or by tracing the progreſs of evolution. The philoſopher of Geneva could not but deduce important conſequences from this fact. He began to reflect upon the nature of the ſeeds, that they were bodies admirably organized, bodies now in miniature what they were ſoon to be at full length; that, like all the parts of the egg taken together, all the parts of theſe bodies conſpire to form an organic whole. He juſtly concluded, that in the ſeeds of peaſe, ſhewn to him by the Daniſh naturaliſt, the plantule exiſted with its lobes, as in the egg, however diminutive, the embryo, with its covers, lies concealed. Hence he thought himſelf authorized to infer, that as the ſeeds of peaſe exiſt before the expanſion of the bloſſoms, and, of courſe, before the aſperſion of the fecundating duſt, ſo the lobes and plantule pre-exiſt in like manner (*a*); the very ſame conſequence which I have above deduced from my numerous obſervations (XXXIX).

(*a*) Palingénéſie, T. 1. page 420, 421.

I may

I may poſſibly be aſked by ſome curious perſons, whether, after having abundantly proved, that the embryos of my plants exiſt before fecundation, or, to uſe an expreſſion equally applicable to thoſe which require the fecundating duſt, and thoſe which do not, before the opening of the bloſſoms, I ever thought of having recourſe to ſome contrivance for rendering them viſible before this period. It is proper to inform ſuch enquirers, that the idea has not only occurred to me, but that I have put it in practice, though indeed without effect. I have taken the ſeeds out of the cloſe and compact bloſſoms, and diſſected them under a powerful magnifier, but have never been able to diſcover any thing more than by a glaſs of inferior power, that is to ſay, an homogeneous gelatinous matter. I doubt whether the embryo might not be withheld from my view, more by its tranſparency, for the jelly was tranſparent, than its ſmallneſs. I boiled ſome ſeed for a little while, and ſteeped others in ſpirit of wine. The jelly was inſpiſſated in both caſes, but the embryo was not rendered conſpicuous. Coloured infuſions have ſometimes been employed by naturaliſts, with the utmoſt advantage, to bring into view thoſe organs of plants, which are otherwiſe inviſible. By theſe means Mr. Bonnet has diſcovered the ſmall veſſels which ariſe from the embryo, and paſs in a ſerpentine direction through the ſubſtance of the lobes (*a*). I put ſeveral ſeeds into differently coloured infuſions, and they acquired, both externally and internally, the colour of the infuſion.

(*a*) Recherches ſur l'uſage dės feuilles.

But

But the embryos were not rendered in the ſmalleſt degree perceptible. Thus unſucceſsful did theſe various trials, as well upon cloſed as expanded bloſſoms, prove, with reſpect to the embryos, nor were they more fortunate with reſpect to the lobes. The ſame thing happened after the falling of the flowers, and the formation of the cavity ſo often deſcribed. The whole advantage gained by my applications aroſe from ſpirit of wine, which inſpiſſated the liquor of the cavity and the embryo at the ſame time, ſo that it might be diſcerned five or ſix days ſooner, as I collected from the examination of other ſeeds of the ſame age left on the plant. This reſult adds confirmation to the truth above eſtabliſhed, that the embryos are not generated, but only unfolded in the ſeeds (XXXVIII, XXXIX). It moreover proves the extreme difficulty, I might ſay, the impoſſibility of obtaining a view of the plantule before the expanſion of the bloſſoms. When ſpirit of wine enabled me to diſcern it, it was yet a moſt diminutive atom, notwithſtanding fifteen days had elapſed ſince the opening of the bloſſom in which it grew. Now the plantule had, doubtleſs, in this interval, paſſed through ſeveral ſucceſſive ſtages of growth. And how prodigiouſly rapid is the growth during this firſt period! How inconceivably ſmall, therefore, muſt the plantule be before the opening of the flowers! Is it not then unreaſonable to hope, that we ſhall be able to diſcern an object ſo exceſſively diminutive?

XLI. The illuſtrious Haller was the firſt who ſhewed, that in birds the fetus

exiſts

exiſts before fecundation. I have extended this diſcovery to different ſpecies of amphibious animals. Theſe facts led me to believe it probable, that the ſame pre-exiſtence takes place in all animals. What has been diſcovered in various individuals of the animal kingdom, has been alſo obſerved in ſeveral ſorts of plants, I have therefore an equal right to draw the ſame concluſion with reſpect to all plants. But if I may expreſs my deliberate opinion on this important topic, I dare not affirm that the embryos, in all caſes, exiſt before the opening of the bloſſoms. Such a deciſion would be precipitate; we may learn from a thouſand examples the variety of Nature, even in thoſe operations which are deſigned to accompliſh the ſame end. I therefore only obſerve, that till the contrary ſhall be proved, we have juſt reaſon to continue in believing pre-exiſtence to be univerſal. Hence we have a new and ſtriking point of analogy between plants and animals, to be added to the many others long known; and hence the ſuſpicion, that theſe two tribes of organized beings compoſe, perhaps, only one immenſe family, receives ſtrong confirmation.

XLII. Baſil and mercury are two plants, which unite with many others, to ſhew the neceſſity of pollen to fecundation. The latter of theſe is particularly deſerving of the reflections of the philoſopher. Female plants of mercury, at no great diſtance from the males, remain ſterile, becauſe the fecundating atmoſphere, if I may ſo ſpeak, of the latter, is not far diſſeminated, on account of the ſmall quantity of pollen produced by them (XXXIII, XXXVI). If the males be brought near

near the females, they bear ſome productive ſeeds (XXXIV); but if the males be placed almoſt in contact with the females, the greateſt part of the ſeeds will adhere and ripen and germinate (XXXV). Should the males be again removed, the females return to their former barrenneſs (XXXVI). This experiment is exactly conformable to one related by Duhamel and Bernard Juſſieu. Thoſe botaniſts having obſerved, that a female turpentine-tree, growing in a garden at Paris, remained conſtantly barren for want of a male, notwithſtanding it flowered every year, determined to bring one near it. The very year they put their deſign in execution, the female bore a plentiful crop of fruit and productive ſeeds. But the following year the male, which grew in a pot, being carried to its former ſtation (*a*), the female relapſed into its accuſtomed ſterility (*b*).

(*a*) Duhamel Phyſique des Arb. T. I.

(*b*) There is growing in the Biſhop of London's garden, at Fulham, a male tree of this ſpecies, and in the botanic garden at Chelſea, a female. Some years ſince an attempt was made to fecundate the female, from which the ſeeds always drop prematurely, by carrying ſome branches cut from the male, and fixing them upon the female. But the experiment, from whatever cauſe, proved unſucceſsful: whether the antheræ had already ſhed their powder, or whether it was ſhaken off in carrying. Plants of the turpentine-tree are ſcarce and valuable; it would therefore be an object, not only of curioſity, but of uſe, to ſucceed in the fecundation of the female; and ſurely the end may be attained, by a little patience and dexterity. Let ſome pollen, for inſtance, be gathered, and carefully ſprinkled upon the female bloſſoms, according to the method ſucceſsfully employed by Mr. Gleditſch, in impregnating the palm. T.

XLIII.

XLIII. The ſucceſsful experiment of Mr. Gleditſch upon the palm, denominated *Chamairops*, by Linnæus, and by Boerhaave *Palma dactylifera major, ſpinoſa, fœmina, folio flabelliformi*, has been much celebrated. This palm, which grew in the Royal Garden at Berlin, and is perhaps ſtill growing there, had never borne fruit, notwithſtanding it was eighty years old. The ſagacious naturaliſt ſuſpecting, that the failure aroſe from the want of a male plant in the garden, and having no opportunity of availing himſelf of the expedient employed by the two Academicians of Paris, had recourſe to the only alternative remaining. He procured ſome pollen from the male of the ſame ſpecies, and ſprinkled it upon ſome of the female bloſſoms. The event correſponded admirably with the views of the curious obſerver. All the flowers that were touched by the pollen produced dates in proper ſeaſon, and from the ſeeds which were planted there ſprang an equal number of young palms; but the other flowers, upon which the influence of the pollen had not been exerted, loſt the greateſt part of their fruit, while it was yet ſmall, and in the little that remained, there was no ſtone, nor did the ſeeds germinate (*a*). One circumſtance attending this artificial fecundation deſerves to be mentioned. The pollen employed by the botaniſt of Berlin was not freſh, but dry; it had been gathered nine days.

When I was making my experiments upon mercury, it did not occur to me to attempt

(*a*) Acad. de Pruſſ. for the year 1767.

artificial fecundation; but I have not the leaſt heſitation in ſuppoſing, that my ſucceſs would have been the ſame as that of the Pruſſian naturaliſt. And as ſuch experiments, under the guidance of a philoſophical mind, cannot but throw great light upon the obſcure ſubject of the fecundation of plants, I exhort botaniſts to undertake them upon mercury, a plant, not confined, like the palm, to a few countries, but widely ſpread over the globe. I could wiſh, that, in the firſt place, if I may be allowed to expreſs my ſlender conceptions, the pollen of the mercury might be carefully examined, in order to diſcover what part of it is the immediate cauſe of fecundation. I have already obſerved (XV), that it conſiſts of veſicles. In theſe veſicles the induſtry of enquirers has detected a ſubtile liquor, containing a multitude of globules. This ſimple obſervation has given riſe to ſeveral diſcordant opinions. Some have ſuppoſed the globules to be the immediate agents of fecundation; others have attributed this effect to the ſubtile liquor; nor have there been wanting others, who have imagined it to be produced by a fluid of exceſſive tenuity, a ſpecies of vital ſpirit, exhaling from the above-mentioned liquor, which ſerves only as a vehicle to the fluid (*a*). A few delicate experiments would perhaps determine which of theſe opinions is the true one. With reſpect to the laſt, we might probably put it to the proof, by occaſioning the exploſion of the veſicles by moiſture, and expoſing the liquor they yield for ſome time to the air. If it

(*a*) Adanſon familles des Plantes, T. 1.

will

will not now fecundate the female bloſſoms of mercury, it would ſeem, that it is effected by ſuch a ſpirit, or at leaſt the moſt ſubtle, active, and volatile part of the liquor, which by this time has evaporated. But ſhould fecundation nevertheleſs take place, inſtead of imputing it to the ſpirit, it muſt be aſcribed either to the liquor or the globules, and then the reſult of my experiments upon animals would be verified with reſpect to plants: it would appear, that the efficient cauſe of fecundation is not the ſubtle, volatile part of the ſpermatic liquor, but the groſs ſenſible portion. Next, in order to learn whether the globules, or the liquor independent of them, produces fecundation, it will be neceſſary to ſeparate the one from the other, and touch different female bloſſoms with each; nor do I think, that a perſon accuſtomed to delicate experiments, would find it very difficult to effect this ſeparation.

XLIV. Mr. Adanſon lays it down as a certain truth, that the ſmalleſt particle imaginable of pollen falling upon the ſtigma, will produce fecundation (*a*). If this was really the caſe, the impregnation of plants would in this particular agree with that of ſeveral amphibious animals, for which I have ſhewn an infinitely ſmall drop of ſeed to be ſufficient. But this renowned Academician muſt allow me to remark, that what he advances is a mere hypotheſis, in ſupport of which he does not bring the ſmalleſt preſumption, much leſs any proof: nor does he offer either preſumption or proof of the exiſtence of

(*a*) L. c.

this

this vital fecundating ſpirit in plants, notwithſtanding he aſſerts, that the embryos of animals are impregnated by a ſpirit of the ſame nature, iſſuing from the ſeed; an aſſertion which my experiments ſhew to be abſolutely falſe. Nothing, however, can be eaſier than to bring this hypotheſis to the teſt of proper experiments, by artificially fecundating mercury. And were ſuch experiments diverſified, like mine upon animals, I have no doubt but that they would furniſh new and curious information.

XLV. The ſeminal liquors of animals and plants poſſeſs one property in common; they retain their virtue for ſome time after they have been taken from their natural reſervoirs. The ſeed of frogs and toads preſerves its prolific power for ſeveral days after it has been taken out of the animals; and the ſame quality was obſerved in the pollen of the palm, ſeveral days after it was ſeparated from the plant (XLIII). But the ſeed of animals becomes in no very long time inert. Are we to ſuppoſe that the ſame thing happens to the pollen after the dropping of the ſtamina? If we conſider the phyſical cauſe which, according to every appearance, deſtroys the virtue of the ſeed of animals, it would ſeem as if the inference was not very juſt. The ſeed of animals becomes unfit for fecundation exactly in proportion as it putrefies; and if it can be preſerved from putrefaction, it continues to be prolific. This ſpecies of corruption does not ſeem ſo much to be apprehended in the pollen, or rather in the attenuated liquor which it contains. From the analyſis given by able naturaliſts we learn,

that

that the liquor is of an oily nature, and refuſes to mix with water (*c*). It appears, therefore, not unreaſonable to ſuppoſe, that, like other vegetable oils, it will keep for a long time; hence ſome may be induced to think that the prolific virtue of the pollen continues ſo much longer than is generally believed, that there is nothing extravagant in imagining it to laſt for many months, nay for a number of years. But notwithſtanding its oily nature, which may ſeem likely to preſerve it from putrefying, the following reaſons lead me to ſuppoſe that it ſoon loſes its prolific power. In order that this liquor may produce its proper effect, and the ſame obſervation is applicable to the globules and the ſpirit of Adanſon (XLIII), it muſt at the time it acts on the ſtigma iſſue from the veſicles, or have been lately diſcharged, for it is natural to ſuppoſe, that by expoſure to the injuries of the air it muſt be diſperſed, altered, and rendered unfit for fecundation. But this diſperſion and alteration muſt unavoidably happen to pollen, that has been ripe for ſome time. According to Mr. Needham's obſervation, which has been verified as well by others as by me, that if the ripe pollen of any plant whatever be examined by the microſcope, we ſhall always perceive ſome veſicles from which the liquor has eſcaped; thoſe that ſtill retain it will explode if they be wetted, like an eolopile ſuddenly expoſed to a ſtrong heat. It is equally agreeable and ſurpriſing to ſee the ſucceſſive exploſion of the veſicles, and the projection of the liquor, a few inſtants after the application

(*c*) Gleditſch. l. c.

of water. From this experiment it appears, that the veſicles remaining after fecundation, whether they float in the atmoſphere or fall to the ground, cannot continue long without exploding and diſpelling their contents, becauſe ſome veſicles will burſt ſpontaneouſly, and the exploſion of the reſt will inevitably be occaſioned by vapour, dew, or at leaſt by rain. Theſe agents will, I am perſuaded, prevent the pollen from retaining its prolific virtue long after the ſeaſon of fecundation. That rain, in particular, is deſtructive of this quality evidently appears from thoſe plants, which, during the time of flowering, are expoſed to violent rains, being either barren or bearing only unproductive ſeeds. This failure is owing to the exploſion of the veſicles before they reach the ſtigma, the ſituation in which they fecundate the embryos contained in the ovarium.

However this may be, to return for one moment to the continuance of the virtue of the pollen, after the ſtamina are withered.—The term ought certainly to be aſcertained both in mercury and other plants. The ſubject is highly intereſting.

XLVI. It has been uſually admitted as a decided point, that fecundation is effected by ſome veſicles, or, according to the common expreſſion, by ſome granules of pollen, which entering through the ſtigma into the ſtyle, advance into its longitudinal duct, till being ſtopped by the narrowneſs of the paſſage, and moreover moiſtened by the juice oozing out from the ſides, they burſt and ſend forth the ſpermatic vapour, which penetrates as far as the ovarium, and impregnates the ſeeds contained

tained in it. But this explanation has not had the good fortune of meeting with Mr. Adanſon's approbation; not that he denies the pre-exiſtence of the ſeed in the ovarium, nor that it is fecundated by the liquor of the pollen, or, as he calls it, by the vital ſpirit reſiding in it. But he pretends that the vital ſpirit does not paſs along this duct to the ſeeds, becauſe in many plants there is no ſuch duct exiſting. He allows, indeed, that in ſome plants, as in ſeveral of the liliaceous tribe, the canal extends from the ſtigma to the ovarium, but he aſſerts that it is otherwiſe in the greater number of other plants, which have their piſtils imperforated. In order, therefore, to explain the manner of fecundation, he imagines that his vital ſpirit, which in ſubtility and activity is equal to the electrical fluid, paſſes along the tracheæ that terminate at the ſurface of the ſtigmata into the placenta, whence it inſinuates itſelf into the ſeed, and fecundates the embryo (*a*). The illuſtrious Bonnet, in a ſenſible and profound Memoir, entituled, *Idées ſur la Fécundation des Plantes* (*b*), mentions the opinion of Adanſon, but appears little ſatisfied with his hypotheſis concerning the *tracheæ*, on account of its being altogether precarious, although it is propoſed by the author as a well-aſcertained fact, and moreover finds great difficulty in admitting that any piſtils are without a duct, and perfectly cloſed. Hence, prompted by his conſtant ardour for the in-

(*a*) L. c.

(*b*) Inſerted in Rozier's Journal for 1774, and reprinted in his works.

veſtigation of truth, he takes occaſion to exhort philoſophical botaniſts to examine this matter afreſh. Although it was not my purpoſe, when I was making enquiries concerning the generation of plants, to enter upon a complete examination of the internal ſtructure of the piſtil, I did, however, occaſionally examine ſome. For this purpoſe I cut the piſtil tranſverſely into ſeveral pieces, and then placed them vertically under the microſcope. Thus I could aſcertain whether they were perforated or not. I found that ſome piſtils were completely perforated from the ſtigma to the ovarium; in others, the canal did not reach above half the length, and ſometimes not ſo far down. Laſtly, in ſeveral other piſtils there appeared no perforation at all. Upon the piſtils of mercury I made no obſervation, which, however, it would be proper to do; and then the validity of that hypotheſis, according to which fecundation is not effected by the paſſage of a ſubtile impregnating ſubſtance along the duct of the piſtil, but along the tracheæ, which terminate on the ſurface of the ſtigmata, may be ſcrutinized. I could wiſh that ſome pollen might be laid upon their ſurface, while care is, at the ſame time taken, that none enters into the aperture of the duct: and that on other piſtils the inverſe experiment might be attempted, that is to ſay, that ſome pollen might be introduced into the opening, while the ſurface remains untouched, and that we might ſuſpend our opinion till we know the iſſue of the trial.

XLVII. But although I could diſcern no duct in the piſtils of ſome plants, I am not, with

with Mr. Adanſon, diſpoſed to think that there are none. While I ſincerely wiſh for knowledge extenſive as his, I ſhould be ſorry to reaſon in the ſame manner. I have found that the veſicles or granules of pollen vary with the ſize of the plant, an obſervation which has been made and publiſhed many years ſince by other naturaliſts. Some are ſo large as to be perceptible by means of a ſingle glaſs; while others are ſcarce diſcerned by the aid of the microſcope. Hence I can believe, without reluctance, that there are ſome which no aſſiſtance can bring under the cognizance of the ſenſes. If we ſuppoſe the duct in the piſtil to be the canal of fecundation, its area will be proportional to the bulk of the granules which are to enter it: if they be large, the paſſage muſt be wide; if they be very ſmall or inviſible, the paſſage will probably be ſo alſo. According to this ſuppoſition then, we ſhall not be able to ſee the duct, ſhould it even exiſt. In order to determine whether my reflection be juſt, it will be only neceſſary to obſerve, whether the granules of pollen are inviſible when the piſtil appears to be without a duct. But ſhould the granules be perceptible, and the piſtils have no canal, as far as the eye can judge, it would hence follow, that this is really the caſe, for it may eaſily happen, that the piſtil ſhall be examined at a time when the duct is either not yet open or has cloſed. Let me at once illuſtrate and prove this propoſition. In the *Sponſalia Plantarum*, which is without doubt one of the beſt diſſertations in the *Amœnitates Academicæ*, the celebrated Linnæus deſcribing the amours of plants

 with

with great complacency, and in the same strain in which another naturalist would describe the amours of animals. He adduces, among other evident marks of the female's appetite for the male, the direction of the stigma towards the pistil and its dilation, like the gaping of a ravenous dragon, longing for the pollen of the male, with which, when it is satiated, it closes. *Gratiola*, these are his expressions, *æstro venereo agitata pistillum stigmate hiat rapacis instar draconis nil nisi masculinum pulverem affectans; at satiata rictum claudit*. He applies terms of like import to various other plants. The reader must by this time perceive, that what I would observe is, that we may possibly miss the exact season of the female's ardour for the male; and then the aperture of the stigma will either be closed after satiety, or not yet opened. Hence we should falsely conclude, that these stigmas are without ducts. A circumstance of the same kind happens to various animals, particularly to some of the amphibious class, as frogs, toads, newts, of which the females have their oviducts quite free and open at the season of their amours; but at all other times they are so narrow, that air can scarce be introduced into them by means of a small syringe. If I should be asked, when the female is most ardent, and when, of course, the ducts are to be sought for, I would answer with Mr. Gleditsch, that at this period the delicate *papillæ* upon the stigma are slightly covered with moisture of the same kind as that which transudes from the vesicles of pollen, for then, according to that author, fecundation takes place.

XLVIII.

XLVIII. I have before obſerved, that the ſurface only of the ſtigma without the orifice might be touched with the pollen of mercury (XLVI). But I ſhould wiſh to know alſo, what would be the conſequence of touching other parts, as for inſtance the ſurface of the ſtile. Fecundation might be alſo attempted upon the leaves and root of female mercury. This idea is not my own, but partly at leaſt Mr. Bonnet's, who obligingly communicated it to me ſome time ſince, that I might put it to trial, but I have been hindered by my other purſuits. Let us hear him in his own words: The author," (who is, as he ſuppoſes, M. Fougeroux, ſee the long note at paragraph XXV), " finiſhes his Memoir with ſome ingenious views, which to him appear worthy to be followed up: I earneſtly wiſh you could undertake this taſk: He would be glad, for inſtance, that attempts were made to fecundate female plants by the root. This idea certainly deſerves to be brought to the teſt of experiment. But the root is at a great diſtance from the flower, and the impregnating ſpirit muſt travel far before it can reach the ovarium. It would be therefore proper to try the ſhorter road of the leaves, and above all of the petals."

XLIX. But it is time to proceed from mercury to other plants, not leſs worthy of the attention of the naturaliſt. In mercury, as well as in baſil, we have ſeen the neceſſity of pollen to fecundation. The pumpion or gourd with ſhield-form fruit, the citron pumpion, hemp, ſpinach, exhibited a phænomenon directly contrary: they produced,

 without

without the influence of the male duſt, ſeeds capable of perpetuating the ſpecies. And in order to be ſatisfied that the duſt had no ſhare in producing the effect, recourſe was had to the moſt vigilant precautions, and the moſt deciſive proofs. The pumpion with ſhield-form fruit flowered in a place many miles diſtant from any plant of the ſame or a neighbouring ſpecies: moreover, the male flowers were all plucked off at their firſt appearance (XX, XXI). The ſame precautions were uſed with reſpect to the other ſpecies of pumpion, with the addition of a third; the acceſs of the external air to the female flowers, was prevented during all the time, in the courſe of which they muſt, in the opinion of botaniſts, be impregnated by the pollen; for this purpoſe the bloſſoms and branches were introduced into glaſs bottles perfectly cloſe (XXXII). Various precautions were likewiſe taken with hemp. In the firſt place, ſeveral female individuals were ſhut up in an apartment, from three weeks before their flowering to the time of the ripening of the ſeeds. Secondly, ſome branches were incloſed in bottles for the ſame ſpace, as in the citron-pumpion. Thirdly, ſome female ſtalks were made to flower ſix weeks before thoſe which grew in the hemp-yards. Laſtly, I aſſured myſelf, that no male bloſſom ever grew upon the females (XXVI, XXVII, XXVIII, XXIX). Precautions of the ſame kind were obſerved with reſpect to ſpinach; ſome female plants were raiſed in a garden, in which there were no males; ſecondly, the pot was completely covered with a glaſs receiver, when the flowers

flowers with piſtils were in bloſſom; thirdly, bloſſoms with piſtils were procured earlier than the uſual appearance of bloſſoms with ſtamina in the gardens and fields: laſtly, I took care to ſatisfy myſelf, that none of the females ever produced any male flower (XXX, XXXI, XXXII). One circumſtance relative to hemp I muſt not omit to repeat; the ſeeds of individuals confined in an apartment, were leſs numerous and ſmaller than of thoſe which grow in the open air. Both theſe defects were ſtill more remarkable on thoſe branches which were introduced into bottles (XXVII, XXVIII). Are we to ſuppoſe, that theſe defects aroſe from want of pollen? I am little inclined to admit ſuch a ſuppoſition; it is far more probable, that they were owing to confinement in an apartment, and what was ſtill more unfavourable, in bottles: hence the plants, and conſequently the ſeeds could not attain the ſame degree of perfection as thoſe, which in the fields enjoyed the benefit of freſh and open air, and were ſubject to the direct influence of the ſolar rays. My opinion is ſtrongly confirmed by thoſe plants which flowered ſix weeks earlier than the others, and, notwithſtanding the abſence of the pollen, produced perfect ſeeds as large, and in as great abundance as thoſe of the hemp-yards, becauſe they were conſtantly expoſed to the kindly influence of the air and ſun (XXIX). From theſe obſervations on hemp, pumpions, ſpinach, compared with others made on baſil, mercury, palm, turpentine, &c. &c. it may in general be inferred, that if a great number of plants require the powder

der of the male, in order to fructify, in many others fructification has no dependence on this powder. And although we are as yet acquainted with a few only belonging to the latter clafs, it will doubtlefs increafe in proportion to the number, application, and fagacity of thofe who fhall employ themfelves in cultivating this branch of the Natural Hiftory of vegetables.

L. But by affirming, that there are plants which bear productive feeds independently of pollen, I am aware, that I fhall incur the difpleafure of all modern naturalifts and botanifts. They will exclaim againft me with the utmoft indignation, and reproach me with having advanced the moft abfurd of all poffible propofitions. I fhall be told, that from the days of Cæfalpinus to the prefent hour, every naturalift of diftinction, Grew, Ray, Camerarius, Morland, Geoffroy, Vaillant, the Juffieus, Duhamel, Adanfon, Bonnet, &c. &c. have admitted the two fexes of plants, and the neceffity of both to fecundation—that the prince of modern botanifts has confecrated an entire effay to the celebration of the amours of plants, and to the defcription of their fexual parts—that upon this diftinction of fex, as upon a folid and immoveable bafis, he has founded the great edifice of his famous *Syftem of Nature*—that in conformity with this theory he defines the bloffoms, *the organs of generation in plants, which are fubfervient to the impregnation of the feeds*—that in the effay, to which I have referred, he fhews, that the duft of the ftamina is not diffufed, till the ftigma of the piftil is in a fit condition to receive its influence

fluence—that the piſtils are always diſpoſed, with reſpect to the ſtamina, in a ſituation favourable for the reception of the pollen. They will bring to my recollection the proof of the neceſſity of the piſtils and ſtamina to fecundation, deduced from experiment; for if the ſtamina or piſtils of hermaphrodite flowers be cut away before the time of blowing, fructification will infallibly be prevented; and the ſame failure is obſerved whenever the ſtamina are converted in petals, or the piſtils are expanded into little leaves. Finally, they will adduce, in full confirmation of theſe aſſertions, the various inſtances of artificial impregnation, ſucceſsfully attempted by different naturaliſts upon various ſpecies of plants, by means of pollen. From all theſe conſiderations, it would ſeem no longer to be queſtioned, that the fecundation of plants, in conſequence of the action of pollen, is one of the moſt univerſal laws of Nature.

LI. Such are the objections which I ſeem to hear urged againſt me by the learned and worthy maintainers of ſexualiſm. I muſt, however, inform them, that I was not unacquainted with theſe arguments, when I made my experiments upon plants. I knew, moreover, that ſome of them were directly oppoſite to experiments related by the northern Pliny; as that, for inſtance, deſcribed by him in the following terms. "Cannabem flores maſculos tantum ferentem, ſi ante divellit ruſtica quam cannabis ſeminifera flores piſtilliferos non aperuerit, nullam aut ſane exiguam portabit ſeminum copiam (*a*). I

(*a*) Spons Plant.

also

alſo had in view an aſſertion, nearly alike, of the illuſtrious Duhamel, in his *Phyſique des Arbres*. *It has been obſerved*, ſays he, *that an inſulated plant of ſpinach produces but a very little ſeed capable of germinating*. When I firſt read this writer, I gave full credit to his aſſertion, for I had no reaſon to doubt of it; and even quoted it in a note on the *Contemplation de la Nature*, tranſlated by me, and publiſhed at Modena, in the year 1770.

Theſe reſpectable authorities ſerved only to render me more diligent, cautious, and aſſiduous in my experiments. They had not ſuch influence as to make me adopt an opinion, contrary to that to which facts pointed: nor ſhall they now hinder me from making, with proper deference, ſuch ſtrictures upon the inſtances juſt quoted, as are ſuggeſted by the diſpaſſionate love of truth. To begin with the fact mentioned by Linnæus, it does not appear that he made any experiment upon hemp; he ſeems merely to have adopted the vulgar notion, according to which, if the male hemp be pulled before the flowering of the female, the latter will produce either no ſeeds at all, or only very few. To ſhew the falſeneſs of this notion, I have only to advert to my experiments, in which every ſtalk of hemp proved fruitful, though they were all kept ſo cloſe, as to be perfectly exempt from the influence of pollen—not to mention the common practice, in the territories of Reggio and Modena, of pulling the male plants at a period, after which the females grow vigourouſly for ſeveral weeks, and bear productive ſeeds. I intreat the learned reader to call to mind the twenty-fourth paragraph, in which this

this practice is described; and also to compare the force of my observations on hemp, with the few contrary expressions of the late Upsal Professor, and then to decide. I give the same answer to Duhamel's intimation concerning spinach. If that illustrious naturalist had deduced the barrenness of this plant when insulated, from experiments of his own, I would have discussed them, and have rendered them all the justice they would have deserved; but he barely mentions the common supposition, which is contradicted by the facts adduced by me.

With respect to the diffusion of the pollen, which was the first of the three arguments in favour of the sexual system, only at a time when the stigma is in a fit condition to receive it; and with respect to the favourable position of the stamina and pistil, the philosopher will immediately perceive, that these are by no means direct proofs of the sexual system, but mere considerations relative to convenience, calculated, if I may employ the expressions of the great Verulam, rather to allure, than to extort assent.

LII. Of the second argument, deduced from the failure of fructification, when the stamina or pistils are either cut away or degenerate, I must take the liberty to observe, that such instances do not prove that these parts are the organs of generation, but merely that they have some concern in this function, as well as many other parts, which yet have never been suspected to be appropriated to generation. The only facts which clearly evince the distinction of sexes in plants, are instances of the failure of fructification in females placed at a diſtance from males, and of artificial fecundation, which was the third argument adduced. But ſhall we conclude, with theſe worthy naturaliſts, that ſuch facts, of which ſo few are known, in compariſon with the immenſe multitude of plants, are ſufficient to eſtabliſh an univerſal diſtinction of ſexes? Is it not, on the contrary, evident, that they have deduced a general concluſion from particular premiſes? They ought at moſt to have ſaid no more than that, conſidering the cloſe analogy which plants of different claſſes, genera, and ſpecies, bear to one another in their properties, and that as a diſtinction of ſexes has been demonſtrated in ſome individuals, it probably extends to others. Thus, from my diſcovery of the fetus's pre-exiſtence in the female of different amphibious animals, and from a like obſervation made on birds by the great phyſiologiſt of Berne, I ſuppoſed that I might juſtly infer, that the ſame pre-exiſtence extends to other animals. After I had found, that the embryos of ſeveral plants exiſt before the appearance of the flowers (XLI), I made the ſame inference with reſpect to all others. But the favourers of the ſexual ſyſtem are not warranted, by ſo inconſiderable a number of facts, to eſtabliſh it as an univerſal law, as others have done, beſides Linnæus, who, without limitation, defines flowers, *the organs of generation of plants, which are ſubſervient to the fecundation of the ſeeds*. Hence he attributes to plants as many males, or huſbands, as there are ſtamina. But before he ſo deciſively determined the number of huſbands, he ought to have been ſure that they perform

form the office. If this far-famed naturaliſt had ſacrificed leſs to nomenclature, and more to the ſpirit of inveſtigation, he would have been led to ſtudy the parts of flowers more philoſophically and more profoundly; hence he would have been better enabled to aſcertain the truth or falſhood of his ſyſtem: but he was in too great haſte to erect his own ſyſtem upon the ruin of Tournefort's; and his eagerneſs has not, perhaps, been highly advantageous to the philoſophy of botany.

LIII. Among various particulars concerning the generation of plants, which I communicated to Mr. Bonnet, in a letter dated October the 18th, 1777, and alluded to in a note, at paragraph XXV, I mentioned ſome experiments, which were then indeed imperfect, but ſeemed to ſhew, that in ſome plants the fecundity of the ſeeds was totally independent of the powder of the ſtamina. Hence I concluded, that botaniſts in general had been guilty of a ſpecies of ſophiſm. Although what I then wrote to my illuſtrious friend was but a ſketch of thoſe experiments which I afterwards undertook, yet he accepted it with complacency, and admitted my concluſion, notwithſtanding his opinion was before different. In his anſwer, dated November the 29th, the ſame year, he ſet forth the reaſons why he had adopted the ſexual ſyſtem, and ſubjoined the following acknowledgment, which amounts to a panegyric on his candour. "I am, however, ſufficiently convinced by your experiments, that all the great naturaliſts whom I have enumerated, as well as myſelf, were deceived. We had all formed a haſty deciſion, and drawn

drawn a general conclufion from particular premifes. We had deduced the neceffity of the pollen in fecundation, from experiments executed on different fpecies of plants; whereas we ought only to have faid, that it feems to follow, from thefe experiments, that in fuch fpecies the influence of the pollen appears to be neceffary to fecundation."

LIV. Such indeed is the fatal rock on which the genius of fyftem-makers ufually fplits.—They illogically deduce general propofitions from particular proofs. Finding that certain parts are common to a number of plants, and that in them they perform particular functions, and have particular properties, they determine, with wonderful facility, that the fame things take place in all other plants: and on thefe parts performing fuch functions, and endowed with fuch properties, they found their fyftem of botany, without perceiving, that before the fyftem can have fuch an unlimited generality as they pretend, they ought to be acquainted with all the plants of the earth. But how many are yet unknown? How many more than are known? With what propriety then can the whole vegetable kingdom be comprehended under one law? Does the organic world afford a fingle law, which can be called univerfal? Have not the laws, which haftily and imprudently were eftablifhed as univerfal, been found not to be really fuch? The learned botanift, Necker, in his *Phyfiology of Moffes* (*a*), ably points out this defect of fyftem-makers,

(*a*) Phyfiologia Mufcorum, Manhemii, 1774.

in

in which he diſtinguiſhes them from obſervers; not indeed with great purity of ſtyle, yet with the utmoſt juſtneſs of ſentiment. "Alterum ſyſtematicum, alterum obſervatorem diſtinguimus. Ille non niſi quibuſdam plantarum ſpeciebus univerſa ſtabilit ſyſtemata: à particulari ad univerſale concludit, i. e. omnibus globi terraquei vegetabilibus eaſdem proprietates ac iis quæ experimentis explorata ſunt, tribuit. Obſervator omnia theoretica rejicit ſyſtemata, ſolis obſervationibus necnon experimentis innixus, Naturam perſcrutans: perfectio botanices ab individuorum ſingulorum inter ſe affinium eorumque identicorum characterum notitia eſſentialiter pendet. Ea proportione notitia hæc acquiretur, qua obſervatorum numerus, qui valde exiguus, augebitur ſyſtematicorumque cumulus minuetur. Certum indubitatumque quod ſyſtematicum ingenium præcipua cauſa ſit cur de modico prefectu botanices dolemus. Syſtemata botanica cum tempore exoleſcunt, quia Natura ac experientia potiſſimum non nituntur."

LV. That Nature ſhould follow a different proceſs in different plants—that ſome ſhould require the influence of pollen in order to multiply the ſpecies, while it is not neceſſary to others, is perfectly conformable to what we every day obſerve in animals to which they are ſo analogous. Numberleſs animals are incapable of multiplying, without the concurrence of both ſexes, or, at leaſt, without the intervention of that liquor, upon which depends the immortality of the ſpecies; as is evident in man, quadrupeds, birds, fiſhes, reptiles, and inſects. But a vaſt num-

 ber,

ber, on the contrary, propagate without ſuch means; as for inſtance, the POLYPES; under which denomination is included an immenſe variety of ſmall animals, inhabiting the bottom both of the freſh water, of ditches, ponds, puddles, and of the ſalt water of the ſea. To theſe may be added, the lice of plants, a moſt comprehenſive claſs; whole armies dwelling on a ſingle individual. The animalcules of infuſions, moreover, agree with the lice of plants and polypes, in the mode of propagating the ſpecies. I have ſhewn, in my *firſt eſſay*, relative to the *natural hiſtory of vegetables and animals*, that many ſpecies are multiplied by a natural diviſion of the body; ſome ſplitting into two portions, ſome into four, ſome into ſix, and others into eight, &c.—that ſome are oviparous, others viviparous, and that all are ſtrictly hermaphrodites, each individual propagating its kind, without needing the concurrence of another. Thus it may, and really does come to paſs in ſome plants, which multiply without the influence of the fecundating duſt.

LVI. One objection, however, may be ſtarted, and it would be inconſiſtent with my impartiality to ſuppreſs it. It may be urged, by the advocates of the ſexual ſyſtem, that although at the time I was making my experiments upon the pumpion with ſhield-form fruit, the citron pumpion, ſpinach, and hemp, I was perfectly certain that the pollen had no acceſs, yet it might previouſly have impregnated the ſeeds. It may be ſaid, that the female individuals might have been fecundated ſeveral years before, and that by this

ſingle

ſingle act, not only the ſeeds of the current year, but thoſe of ſeveral ſucceſſive generations, might have been fecundated.

This objection is only a repetition of what was modeſtly ſuggeſted by the illuſtrious Trembley, to Mr. Bonnet, when he made his celebrated diſcovery of the hermaphroditiſm of the lice of plants. *Who knows*, ſaid that naturaliſt, ſo cautious and deliberate in his deciſions, *but that one copulation may ſerve for many generations* (*a*)? Although my excellent colleague acknowledges the queſtion to be altogether gratuitous, yet, as it was put by a Trembley, he was anxious to repeat his obſervations; and he juſtly concludes, that the ſuſpicion is entirely removed by the production of ten ſucceſſive generations without copulation (*b*). With reſpect to my plants, I muſt frankly acknowledge, that I did not obtain from any of them more than three generations without the intervention of pollen. Nor did the nature of the thing allow me to ſatisfy my curioſity ſo ſoon as Mr. Bonnet, for he, in the ſhort ſpace of three months, obſerved ten ſucceſſive generations; whereas not leſs than ten years muſt have paſſed, before I could have obtained an equal number in the ſubjects of my experiments. I have not, therefore, been able to derive ſuf-

(*a*) Bonnet Corps Organ. p. 311. Ed. in 4to.

(*b*) Notwithſtanding Mr. Bonnet has diſcovered and publiſhed this fact ſo many years ago, and notwithſtanding it has met with no oppoſition from any obſerver, Linnæus advances, in his *Syſtema Naturæ*, and in the laſt edition of 1767 repeats, this ſcandalous propoſition. " A copula parentum fœcundas naſci filias, neptes, proneptes, abneptes aſſeverant entomologi."

ficient information from this ſource; yet I can adduce conſiderations weighty enough to overturn the objection. In the firſt place, all Nature does not afford a ſingle inſtance, wherein one act of fecundation ſerves for ſeveral generations. The only caſe, concerning which any ſuch ſuſpicion has ariſen, has been abundantly cleared from it. Secondly, we have inſtances of many ſucceſſive generations, without copulation or fecundation, as in the polype with arms. From the body of this ſpecies ſprout ſmall polypes, which, during their evolution, produce other ſmaller polypes, and theſe again others ſtill ſmaller, and ſo on: hence we have at laſt a ſeries of generations, growing one out of another, and all ſupported by the mother polype, of which we are certain, that ſhe has never had any ſort of commerce with her fellows; becauſe ſhe was torn, while yet very ſmall, from the body of another, and ever afterwards kept in the moſt perfect ſolitude (*a*). Since, therefore, we obſerve ſo many ſucceſſions in animals, independently of the action of any ſeminal principle, why may not, or rather why muſt not, the ſame thing take place in ſome kinds of plants? We have no fact which proves a number of ſucceſſive generations in any organized body, to be the effect or the reſult of an antecedent act of fecundation.

LVII. Although the many facts which I have related, compel me to reject the action of the powder of the males in ſeveral plants, yet I dare not altogether deny the poſſibility

(*a*) Trembley, Polypes d'eau douce.

of

of fecundation. What I mean to infinuate is, that in the courfe of my frequent meditations upon this important fubject, a doubt has arifen in my mind, whether the feeds in the ovarium may not be fecundated by fome feminal principle refiding in the piftil. I have frequently feen upon the ftigma of certain plants, a kind of powder exceedingly like that of the ftamina, though I had at the fame time clear proofs, that the latter had not yet burft from the antheræ. But I did not, I muft own, make any careful obfervations on this fubject. My furmife was however ftrengthened by reading in Kölreuter, not without furprize, that the properties of the feminal powder belonging to the ftamina, are exactly the fame as thofe of the matter furnifhed by the piftil. Now I have fometimes doubted, whether the duft of the ftigma may not produce the fame effects on fome plants, as that of the ftamina on others. But this is a mere fufpicion, which it would give me great pleafure to fee confirmed or deftroyed by fome able obferver. And as I am fpeaking of the ftigma, let me add, that I wifh this organ were more carefully examined, than it has ever yet been; great attention fhould be paid, in order to determine whether it is imperforated or not in fome plants (XLVI, XLVII). Such refearches, united with others relative to the ftamina, would contribute to diffipate the mift which covers the fecundation of plants. As I have derived great advantage in my attempts to illuftrate this function in animals, from artificial fecundation, I cannot but again recommend the fame means; nor do I

 doubt,

doubt, but that they will throw as much light upon plants as upon animals.

LVIII. By theſe exhortations, ſuggeſted by an unfeigned deſire of attaining truth, and extending the limits of botannical knowledge, I wiſh to induce naturaliſts to ſtudy in a more philoſophical manner the natural hiſtory of plants. The ligneous ſpecies have, it is true, been much illuſtrated by the profound enquiries of Grew, Malpighi, Hales, Duhamel, and Bonnet; but the herbaceous kinds ſo much exceeding the others in number, and ſo deeply intereſting to mankind, on account of the ſuſtenance they afford, have been hitherto almoſt totally neglected. We have at moſt a bare nomenclature, in which their different parts are deſcribed according to the ſyſtem adopted by the author. I do not on this account condemn nomenclatures; the neceſſity of them is evident, ſince it is impoſible to enter upon a rational inveſtigation of plants before we are acquainted with them; and we cannot be acquainted with them without the ſcience of names (*a*). I obſerve only, that nomenclature gliding

(*a*) Beſides being obliged to learn the nomenclature, (whence it may be called the grammar of natural hiſtory, and thoſe who teach it the grammarians) we are under the neceſſity of uſing it when we write, if we wiſh to be generally underſtood. Thus, in treating of plants and animals, unleſs they happen to be the moſt common of all, it would appear, that at preſent we cannot do without Linnæus. If, at leaſt, we make uſe of the names in his *Syſtema Naturæ*, whatever glaring defects that work may contain, we are ſure of being underſtood by all Europe. I ſaid *plants* and *animals*; for *foſſils* we uſe Cronſtedt and Wallerius, as the claſſical authors.

along

along the ſurface of things, is incapable of furniſhing ſuch information, as will ſatisfy the curioſity of the profound enquirer, and advance our knowledge of the vegetable kingdom. The bodies of Nature are not ſimple, but exceedingly complex; Muſchenbrock juſtly compares them to a clock, conſiſting of many wheels, and encloſed in a caſe, which prevents us from obtaining a view of the ſize and mutual dependency of the wheels, and the various actions of the ſprings. To ſee, therefore, the internal parts, and eſtimate them properly, it becomes neceſſary to open the caſe: we muſt do the ſame with reſpect to natural bodies, and not be contented with barely inſpecting the outſide, but endeavour to penetrate further, and inveſtigate the wonders lying within. When the firſt mineralogiſts characteriſed the productions of the foſſil kingdom, by difference in colour, in tranſparency, opacity, by roughneſs or ſmoothneſs, by their granulated or fibrous ſtructure, &c. they held out only ſuperficial, vague, and, for the moſt part, fallacious notions. Before they could be properly underſtood, it was neceſſary that chemiſtry ſhould ſearch theſe productions by its analyſis. What this ſcience has done for foſſils, anatomy has performed for animals. We cannot but acknowledge the greateſt obligations to Swammerdam, Redi, Valliſneri, Reaumur, Lyonet, Daubenton, and, above all, in the claſs of inſects, to the immortal Malpighi, whoſe diſſertation on the ſilk-worm is a tiſſue of diſcoveries, and may be juſtly conſidered as alone ſuperior to all the nomenclatures of inſects hitherto publiſhed. I could wiſh that the ſame attention, which ſo many able naturaliſts have beſtowed upon reſearches concerning foſſils and animals, was paid to herbaceous plants. Their œconomy, the principal object of natural hiſtory, ought, above all, to be the aim of our enquiries. But it will be impoſſible to fulfil ſuch an intention, till a careful examination has been made, both of their internal and external ſtructure (*a*). I am aware, that reſearches of this nature require quickneſs of apprehenſion, pertinacity of attention, fertility of reſources, exactneſs of judgment, the utmoſt vigilance in noticing the phænomena, and no leſs ſagacity in diſtinguiſhing them; theſe requiſites do not ſeem indiſpenſably neceſſary in the learned nomenclator, who is generally conſpicuous chiefly for memory; and this is the true reaſon why, at a time when we are rich in nomenclators, we are poor, nay beggars, in obſervers. But it is only by ſuch laudable exertions, and ſuch valuable accompliſh-

(*a*) Thoſe who, from a deſire to ſerve the public, and to contribute to the progreſs of human knowledge, ſhall apply to the ſtudy of plants, and indeed of minerals and animals alſo, would do well, in my opinion, to follow the example of Erxleben, a late celebrated academician of Gottingen. Mr. Kœſtner, Preſident of that reſpectable academy, draws, in his elogy, the following exemplary character of him, and ſubjoins a very juſt reflection. "Hiſtoriam Naturalem, quam vocant, judicabat (Erxleben) animalium, plantarum, foſſilium, non catalogum eſſe, ſed phyſicam. Itaque horum corporum ſtructuram, analyſim, proprietates, uſus docuit, non neglectis tamen notis quarum ope velut in indices poſſent referri. Quos indices ſolos qui memoriæ mandant aut ad evolvendos illos manus habent exercitatas, fruſtra ſibi perſuadent, librum ipſum Naturæ ſe tenere."

ments,

ments, that we can hope to advance any farther in this noble ſcience, and to increaſe the ſtores of uſeful knowledge. I cannot, therefore, but ſet an higher value upon the genius of thoſe naturaliſts, who confine their attention to ſome part of natural hiſtory not yet ſufficiently elucidated, than upon the art of thoſe, who only employ themſelves about nomenclatures; and I have the pleaſure of agreeing in opinion with thoſe great men, whoſe moſt earneſt wiſh is for the greateſt poſſible improvement of ſcience, and who, while they recommend it, with patriotic zeal, to others, advance it themſelves in their immortal productions. Not that theſe illuſtrious writers deſpiſe nomenclature, which is ſo neceſſary, that it may be called the key of the three kingdoms; but they conſider the purſuits of nomenclators as not of the moſt uſeful tendency, becauſe the deſire of comprehending, in voluminous nomenclatures, like ſo many Briareuſes, the whole of Nature, as more than one have attempted, ſeems to them equally preſumptuous and puerile. If Men would go about to diſcover things, and not words; if they would inveſtigate Nature, and bring to light ſome part of her inexhauſtible ſtores, they would render far greater ſervice to the ſcience which they cultivate, and deſerve better of the republic of letters. Leſt this repreſentation ſhould appear exaggerated, I will adduce, from among many reſpectable authorities which are at hand to confirm it, thoſe of the incomparable d'Alembert, and of the ſublime contemplator of Nature. "We by no means wiſh to reſemble that tribe of naturaliſts, who have been ſo juſtly cenſured by a moa modern philoſopher; inceſſantly employed in dividing the productions of Nature into genera and ſpecies, they ſpend in ſuch labours the time which would much better be dedicated to the ſtudy of their properties." Such are the expreſſions of the former in that moſt maſterly production, the *Preliminary Diſſertation to the Encyclopædia*. I ſhall conclude with the words of the latter. "What ought we to think of thoſe pompous *nomenclatures*, which are arrogantly offered to us as the *Syſtem of Nature*? The idea of a ſcholar, who attempts to form the index of a large folio, of which he has read only the title and the firſt pages, immediately ariſes in my mind. And are we in poſſeſſion of even the firſt pages of the book of Nature? How many paſſages do they contain which we do not underſtand, and of which the hidden meaning probably contains intereſting truths? I wiſh not to extirpate nomenclators; they ſtrive to give our knowledge ſome arrangement; but I will venture to ſay, that a mere nomenclator will never make any great diſcovery. I will add, that I ſet an higher value upon a good treatiſe concerning a ſingle inſect, than upon a whole entomological nomenclature.——Meditate the admirable hiſtory of the polype, peruſe the fine memoirs on inſects, and then compare the uſefulneſs of theſe productions with that of the moſt boaſted nomenclatures. Of which would you ſooner chuſe to be the author? Which, think you, imply moſt ſagacity, genius, and invention, and have the greateſt tendency to improve anatomy and natural hiſtory? We ought, in my opinion, not to be ſo anxious to make

make the catalogue of human knowledge, as to increaſe it. Let us collect more materials before we think of erecting a temple to Nature. She will elſe refuſe to dwell in it. Inſtead of correſponding to her majeſty, it will only be proportional to the meanneſs of the architect (*a*)."

(*a*) Contemplation de la Nature.

END OF DISSERTATION III.

APPENDIX.

AN ABRIDGEMENT OF MR. DEMOUR'S OBSERVATION IS TO BE FOUND IN THE FRENCH MEMOIRS FOR 1741, p. 28. IN THE MEM. FOR 1778, PRINTED IN 1781, p. 13, WE FIND THE ACCOUNT IN HIS OWN WORDS. THE TITLE IS, " OBSERVATION DE DEUX ANIMAUX, DONT LE MALE ACCOUCHE LA FEMELLE.

THE eulogies of Naturaliſts upon the induſtry of animals are boundleſs. It has been pretended, that man is indebted to them for the uſeful arts; and ſome are extravagant enough to affirm, that even the moſt abſtracted ſciences have been borrowed from brutes (*a*). According to Pliny and ſeveral other naturaliſts, the idea of bleeding was ſuggeſted to phyſicians by the hippopotamus; and that of injection by the ſtork, which, on account of the great ſervices it rendered them, was formerly worſhipped by the Egyptians. The bear and the dog pointed out the advantage of emetics; the former by ſwallowing ants, the latter by eating the leaves of dog's graſs. Happily for

(*a*) *Giovanni Bonnifacio*, l'arti liberali e mechaniche, come ſiano ſtate dagli animali irrationali, agli Huomini dimoſtrate.

medicine,

medicine, the fact recorded in this Memoir, was unknown to these naturalists, otherwise they would not have failed to ascribe the origin of the art of midwifery to the animal in question.

One evening during the dog-days, I observed near some steps by the great bason in the king's garden, in which I at that time occupied the office of demostrator, and keeper of the cabinet of Natural History, two land toads of the smaller species (*a*) in the act of copulation. I saw that the male frequently moved his hind legs; a desire to discover the cause of these movements led me towards the place, where I was surprized by two phænomena, which I believe have not been hitherto noticed. The first was the difficulty with which the female brought forth her eggs, which was such that she absolutely required foreign aid, what no other animal with which we are acquainted does. The second phænomenon was the efforts made by the male to extract the eggs with his hind feet, and his thus performing the office of an accoucheur.

I could not conceal my satisfaction at a spectacle so uncommon. My attention redoubled, and I seated myself quietly on the ground, in order to observe more accurately; and especially to watch whether the male impregnated the eggs as he extracted them.

To understand the operation completely, it must be remarked, first, that the fore as well as hind feet of these animals are divided into several toes, by the help of which

(*a*) Rubeta minor. Gesneri.

the

the male draws the eggs out of the vent of the female.

Secondly, that theſe animals copulate like frogs, i. e. the male gets upon the female's back, and embraces her with his fore feet. In the frog the feet are long enough to claſp the female completely, whereas in the toad they reach only to the ſide of the breaſt, againſt which the animal preſſes with ſuch force, that an ecchymoſis often takes place before he quits his ſituation.

I muſt obſerve in the third place, that the eggs of this ſpecies of toad conſiſt of a ſtrong membranous cover, within which the embryo is contained. The eggs are about two lines long, and of an oblong ſhape; they are connected by a ſhort and ſtrong cord. A pretty juſt idea of theſe eggs may be conveyed, by comparing them to a necklace, of which the beads lie at about half their own length from each other. They are diſcharged at the vent, becauſe the receptacle in which they were contained, opens into the lower part of the rectum.

There is reaſon to ſuppoſe, that the firſt egg is expelled by the exertions of the female, and that the male extracts the reſt. He performs his office with an addreſs that one would not expect in an animal naturally ſo aukward. When his motions drew my attention, he had extracted the ſecond, and was endeavouring to extract the ſucceeding egg. The firſt was fixed between the toes of the right foot, by the filament which connects it with the ſecond, and it was by ſtretching out the foot, that he drew them from the anus of the female, which continued

tinued motionleſs during the whole time. He tried to lay hold of the cord with his left foot, and made ſeveral attempts before he ſucceeded. My preſence undoubtedly ſometimes drew off his attention: for he would now and then ſtop ſuddenly, and look at me with an air expreſſive either of inquietude or fear: then he would reſume his employment with freſh alacrity, and ſoon relax again ſo much in his ardour, as to ſeem irreſolute whether he ſhould proceed or not. The female alſo teſtified her diſquiet by movements that diſturbed the operations of the male, but ſhe never ſeemed deſirous of returning to the ſtation under the ſtone which ſhe had quitted.

At laſt, whether from my ſilence and immobility, or from the urgency of the caſe, the female became ſtill, and the male reſumed his employment: he ſoon laid hold of the cord with his left toes, and now drew it out with both feet, by extending them gently. When he had ſtretched them out as far as he could, he withdrew the left foot, without quitting the eggs that adhered to it, and continued to extract with the right alone. But here his difficulties recurred a ſecond time. The portion of cord that lay between the middle fingers of the right foot, often prevented him from getting hold of a new part of the cord with that foot. He made ſeveral efforts without ſucceſs, and ſtopped of a ſudden oftener than once. I was afraid, that the movements of my head were the cauſe of theſe interruptions, and I then ſat motionleſs, and held in my breath. Sometimes I was inclined to attribute them to the difficulty of the

the operation itſelf; this tempted me to aſſiſt the animal, but I forbore, for fear of breaking off the proceſs. My attention had hitherto been divided between two objects; while, on the one hand, I admired the dexterity of the male, I watched on the other, whether he bedewed the eggs with ſemen as he extracted them, but as yet I could not ſee any appearance like this, though it ſeemed abſolutely neceſſary to the fecundation of the eggs, and was, at the ſame time, the chief object of my curioſity: I thought the want of light might prevent me from ſeeing it, and therefore, at all hazards, ſet them on my hand, that I might inſpect them more cloſely.

This change of ſituation put a ſtop to the procedure of the male for a few moments; but the urgent neceſſity for delivering the female, doubtleſs made him collect courage to reſume his employment. I now tried only to obſerve, whether the male impregnated the eggs as he drew them out; but notwithſtanding all my ſolicitude to ſee a phænomenon, of which the water-newt had before furniſhed me with an example (*a*), I could not perceive any thing like what I expected. The evening now cloſed in, and after obſerving them for about half an hour, I was forced to ſet them down in the place from which I had taken them.

(*a*) The female newt is impregnated without coition. The male keeps at about the diſtance of an inch above the female, and throws out the ſeminal liquor upon her ſides, which renders the water a little turbid. This obſervation is to be found at the end of the firſt volume of my tranſlation of the Edinburgh Eſſays.

My meeting with theſe animals was one of thoſe fortunate occurrences, of which a naturaliſt alone can eſtimate the value. It afforded me the view of a fact, ſingular and hitherto I believe unnoticed. The aſſiſtance yielded by the male I ſaw diſtinctly; but it was quite otherwiſe with reſpect to the other part of the obſervation. I could not, with all my attention, perceive any thing like what Swammerdam ſaw in frogs, viz. that after forty days of coition the male impregnates the eggs as the female diſcharges them, by bedewing them with ſemen.

Though I could not perceive this irroration, yet there is reaſon to ſuppoſe, not only that it is neceſſary for the fecundation of this ſpecies of fry, but alſo that it takes place in the ſame manner as in the frog. The exact conformity between the ſtructure of the internal parts of theſe animals, between their mode of coition and their embryos, which in each kind paſs through the ſtate of tadpole before they attain the form of frog or toad, ſtrongly confirms this conjecture. I ſhould therefore ſuppoſe, that the male performs this function at ſome other time. He probably, as well as the frog (*a*), bedews all the eggs at once, which he can not accompliſh when he has them entangled about his feet. In truth, the waſte would be too great if he impregnated them ſucceſſively, and in this caſe a great quantity of ſemen would fall on the ground, and my hand, upon which the male operated

(*a*) How far this ſuppoſition is to be admitted, the reader may learn from the firſt of the preceding diſſertations. T.

for

for about a quarter of an hour, muſt have received ſome.

This obſervation apprized me of a miſtake I had formerly committed. During the two laſt years of M. Duverney's life, I aſſiſted him in his anatomical labours; and I had once occaſion to ſeek by candle-light for newts, which the heat and drought of the ſeaſon confined in their places of retreat in the day-time. Near a reſervoir, conſiſting of a barrel that projected eight or ten inches above the ſurface of the ground, I perceived a toad of the ſpecies above-mentioned, with a bundle of eggs on his back. The animal traverſed round the barrel to no purpoſe; the edge was too high for him to diſcharge his burden into the water. I took it up with an intention to relieve its diſtreſs, by throwing it into the water; in an inſtant I felt ſomething move in my hand, and upon bringing the light near I perceived ſeveral little tadpoles juſt come out of their envelope; they were very lively, and as full formed as tadpoles of frogs that have loſt their tail, and which conſequently have quitted their cover fifteen days. The cord of eggs was ſo entangled in the hind feet, that the animal could make but very ſhort ſteps. At that time I ſuppoſed it to be the female, but the obſervation which I have now related proves, that it muſt have been the male which is charged with the cares that attend incubation. Nature, in the preſent inſtance, has divided the toil that relates to the progeny equally between the two ſexes. The female has the burthen of the eggs before delivery, and likewiſe ſupports the male upon her back, probably for forty days, as in the frog and water toad. The male, in his turn, delivers the female, and carries the eggs on his back till the time of the exclusion of the tadpoles, which happens sooner or later, according to the season, and he then deposits them in a proper place: He throws himself into the first water he meets, and is frequently the victim of paternal attention. If he gets into a place whence he cannot easily find his way out, he perishes in a few days, as I have seen in many instances. This species, which Ol. Jacobæus considers as the *Rana phrynoides* of Gesner, and which some authors call *Rubeta minor*, in order to distinguish it from the common toad, which they denominate *Rubeta major*, is incapable of living in water after he has once quitted it. He passes the former part of his life in water, and the latter upon dry land. This, according to Gesner, was known to the ancient naturalists, who assert the same thing of the common frog.

The species here mentioned quits his envelope in the form of a tadpole without a tail; at this time it has gills, and lives in water like fishes: there it continues till the gills begin to be effaced, which happens as soon as the fore-feet burst through the membrane which contained them: when the animal can walk or jump it seeks a different element, and quits the water before its tail is entirely effaced.

DIS-

Discoveries on the sex of bees, explaining the manner in which their species is propagated; with an account of the utility that may be derived from those discoveries by the actual application of them to practice. By Mr. John Debraw, apothecary to Addenbrook's hospital at Cambridge, and member of an oeconomical society in the principality of Liege, in Westphalia. Communicated by the Rev. Nevil Maskelyne, B. D. F. R. S. and astronomer royal.

Read Nov. 21, 1776.

THE republic of bees has at all times gained univerſal eſteem and admiration: their culture, an object ſo worthy of our attention, has attracted and ſtill does engage that of many of the learned, and has arrived at a conſiderable degree of improvement of late years; but their mode of propagating their ſpecies, ſeems to this day to have baffled the ingenuity of ages in their attempts to diſcover it. The moſt ſkilful naturaliſts have been ſtrangely miſled in their opinion, that the bees, as well as the other tribes of animals, are perpetuated by copulation; though they acknowledge that they have never been able to detect them in the act.

Pliny, who was likewiſe of the ſame opinion, that in this particular they do not differ

 from

from other animals, obſerves, "Apium coitus viſus eſt nunquam." Swammerdam, that ſagacious obſerver, having never been able to diſcover it, entertained a notion, that the female or queen bee was fecundated without copulation; that it was ſufficient for her to be near the males; that a vivifying *aura*, exhaling from the body of the males, and abſorbed by the female, might impregnate her eggs. At laſt the incomparable Reaumur thought he had in a great meaſure removed the veil, and brought their manner of generating nearly to a proof. This part of phyſics has been the principal object of my reſearches for ſeveral years paſt, having been inſenſibly engaged in it by the pleaſure I took in ſo curious an enquiry; and although this purſuit has been attended with more difficulties and embarraſſments than can be well imagined, I have not been diſcouraged, and have carefully avoided launching into conjectures. To introduce a new ſyſtem in the doctrine of bees, which in a great meaſure contradicts all former received opinions, requires, previous to its appearance, every ſanction the various experiments, ſucceſsfully repeated, can poſſibly give it. The reſults of thoſe experiments, made in all glaſs-hives, which carry with them an intire evidence, afford ſufficient reaſons to aſſert, that bees belong to that claſs of animals among which, although they have ſexes, a true copulation cannot be proved; and that their *ova*, like the ſpawn of fiſhes, moſt probably owe their fecundation to an impregnation from the males, as will appear in the ſequel of this narrative.

I am

I am not a little pleaſed to find, that the celebrated Maraldi had ſuch a notion, and I lament his neglecting to confirm it. He ſays, in his *Obſervations upon Bees*, in the Hiſtory of the Academy of Sciences for the year 1712, p. 332: "Nous n'avons pû découvrir juſqu' à preſent de quelle maniere ſe faite cette fécondation, ſi c'eſt dans le corps de la femelle, ou bien ſi c'eſt à la maniere des poiſſons, aprés que la femelle, a poſé ſes œufs: la matiere blanchâtre dont l' œuf eſt environné au fond de l'alvéole peu de temps aprés ſa naiſſance, ſemble conforme à la derniere opinion, auſſi-bien que les remarques faites pluſieurs fois d'un grand nombre d'œufs qui ſont reſtés inféconds au fond de l'alvéole autour deſquels nous n'avons point vû cette matiere."—"We never yet were able to diſcover in what manner this fecundation is performed; whether it is in the body of the female, or whether it is after the manner of fiſhes, after the female or queen-bee has depoſited her eggs: that liquid whitiſh ſubſtance, with which each egg is ſurrounded at the bottom of the cell, a little while after its being laid, ſeemingly eſtabliſhing this laſt opinion, as well as the frequent remarks made of a great number of eggs remaining barren in the cell, round which we could not ſee the above-mentioned whitiſh ſubſtance."

This ingenious naturaliſt, by a nice examination of the ſtructure of the drones, had, as well as Swammerdam, diſcovered ſome reſemblances to the male organs of generation; and from thence conjectured, they were the males of the bee-inſect; but he owns, with the reſt, that he never could diſcover them in the act of copulation.

Having ſtood the trials of ſo many prying eyes in every age, the bees, as has been obſerved by an ingenious author, had gained the character of an inviolable chaſtity, till Reaumur blaſted their reputation. He makes the queen no better than a Meſſalina (*a*); though he could ſee no more than what would raiſe a mere jealouſy or generate ſuſpicions.

In order to be the better underſtood in the relation of my own experiments on the fecundation of bees, I here premiſe the outlines of the opinions adopted by the above-mentioned naturaliſts on that head. They aſſert, that the queen is the only female in the hive, and the mother of the next generation; that the drones are the males by which ſhe is fecundated; and that the working bees, or bees that collect wax on the flowers, that knead it, and form from it the combs and cells which they afterwards fill with honey, are of neither ſex.

But of late Mr. Schirach, a German naturaliſt, has given us a very different view of the claſſes that conſtitute the republic of bees, in an ingenious publication in his own language, under the title of *The Natural Hiſtory of the Queen of the Bees*, which has been ſince tranſlated into French; an account of which has been given in the Monthly Review, from which I beg leave to relate the author's doctrine with regard to the working-bees only; the quality and functions of the drones being

(*a*) Vid. Juvenal, Sat. vi. ver. 128.

points

points which do not appear to be yet ſettled by Mr. Schirach himſelf. He affirms, that all the common bees are females in diſguiſe, in which the organs that diſtinguiſh the ſex, and particularly the *ovaria*, are obliterated, or at leaſt, through their exceſſive minuteneſs, have not yet been obſerved: that every one of thoſe bees, in the earlier period of its exiſtence, is capable of becoming a queen-bee, if the whole community ſhould think proper to nurſe it in a particular manner, and raiſe it to that rank. In ſhort, that the queen-bee lays only two kinds of eggs, viz. thoſe that are to produce the drones, and thoſe from which the working-bees are to proceed.

The trials made by Mr. Schirach ſeem to evince the truth of his concluſions in the moſt ſatisfactory manner, ſingular as they appear to be at firſt ſight; and indeed in my own judgment, from the conſtant happy reſult of numerous experiments, which I began near two years before Mr. Schirach's publication, and repeated every ſeaſon ſince, I am enabled to pronounce on their reality.

Chance, I own, befriended me in that diſcovery, whilſt I was moſt anxiouſly endeavouring to aſcertain the uſe of drones. It was in the ſpring of the year 1770, that I, for the firſt, diſcovered what Maraldi had only conjectured, I mean the impregnation of the eggs by the males; and that I was made acquainted with the difference of ſize in the drones or males, obſerved by Maraldi in his *Obſervations upon Bees*, inſerted in the Hiſtory of the Royal Academy of Sciences for the year 1712, p. 333, in theſe words:

"Nous

"Nous avons trouvé depuis peu une grande quantité de bourdons, beaucoup plus petits que ceux que nous avions remarqué auparavant, & qui ne ſurpaſſent point la grandeur des petites abeilles; de ſorte qu'il n'auroit pas été aiſé de les diſtinguer dans cette ruche des abeilles ordinaires, ſans le grand nombre que nous y en avons trouvé. Il ſe pourroit bien faire que dans les ruches où l'on n'a pas trouvé de gros bourdons, il y en eût de ces petits, & qu'ils y aient été confondus avec le reſte des abeilles, lorſque nous ne ſavions pas encore qu'il y en eût de cette taille."—"We have of late found a great quantity of drones, much ſmaller than thoſe we had formerly obſerved, and which do not exceed in ſize the common bees; ſo that it would not have been eaſy to diſtinguiſh them in that hive from the common bees, had not the quantity of them been very conſiderable. It might certainly have happened, that in thoſe hives, where we have not been able to diſcover large drones, there were a great number of thoſe little ones, which may have been intermixed among common bees, when we were yet ignorant that any ſuch ſmall drones were exiſting."

Reaumur himſelf, p. 591, of his Natural Hiſtory of Inſects, ſays, "We have likewiſe found drones that were no bigger than the common bees."

They have notwithſtanding eſcaped the obſervation of Mr. Schirach, and of his friend Mr. Hattorf, member of an academy in Luſatia, who, in a memoir he preſented in the year 1769, annihilates entirely the uſe of drones in a hive; and advances this ſingular opinion,

opinion, that the queen-bee of a hive lays eggs which produces young ones, without having any communication with the drones. For what purpoſe ſhould wiſe Nature then have furniſhed the drones with that large quantity of ſeminal liquor? To what uſe ſo large an apparatus of fecundating organs, ſo well deſcribed by Reaumur and Maraldi?

But I beg leave to remark, that thoſe gentlemen ſeem to have drawn too haſty concluſions from their experiments, in rejecting the drones as bearing no ſhare in the propagation of thoſe inſects. Their obſervations, that hives are peopled at a time of the year when there are no drones in being, is no ways concluſive; as it is evident, that they had ſeen none but drones of a large ſize, their ſilence on the difference in the ſize of them juſtifying my remark. But to reſume the narrative of my experiments: I had watched my glaſs-hives (*a*) with indefatigable attention from the moment the bees, among which I had taken care to leave a large number of drones, were put into them, to the time of the queen laying her eggs, which generally happens the fourth or fifth day. I obſerved the firſt or ſecond day (always before the third) from the time the eggs are placed in the cells, that a great number of bees, faſtening themſelves to one another, hung down in the form of a curtain, from the top to the bottom of the hive, in a ſimilar manner they had done before at the time the queen depoſited her eggs; an operation which

(*a*) Glaſs-hives were uſed in preference to boxes, for a purpoſe too obvious to need explaining.

(if

(if we may conjecture at the inſtincts of inſects) ſeems contrived to hide what is tranſacting: be that as it will, it anſwered the purpoſe of informing me that ſomething was going forward. In fact, I preſently after perceived ſeveral bees, the ſize of which, through this thick veil, (if I may ſo expreſs myſelf) I could not rightly diſtinguiſh, inſerting the poſterior part of their bodies each into a cell, and ſinking into it, where they continued but a little while. After they had retired, I ſaw plainly with the naked eye a ſmall quantity of a whitiſh liquor left in the angle of the baſis of each cell, containing an egg: it was leſs liquid than honey, and had no ſweet taſte at all. Within a day after, I found this liquor abſorbed into the embryo, which on the fourth day is converted into a ſmall worm, to which the working-bees bring a little honey for nouriſhment, during the firſt eight or ten days after its birth. After that time they ceaſe to feed them; for they ſhut up the cells, where theſe embryos continue incloſed for ten days more, during which time they undergo various changes too tedious here to deſcribe.

To evince the reality of this obſervation, and to prove that the eggs are fecundated by the males, and that their preſence is neceſſary at the time of breeding, I proceeded to the next experiments. They conſiſted in leaving in a hive the queen with only the common bees, without any drones, to ſee whether the eggs ſhe laid would be prolific. I accordingly took a ſwarm, ſhook all the bees into a tub of water, and left them in it till they were quite ſenſeleſs, which gave me an oppor-

opportunity to diſtinguiſh the drones without any danger of being ſtung. After I had recovered the working-bees and their queen from the ſtate they were in, by ſpreading them on brown paper in the ſun, I replaced them in a glaſs-hive, where they ſoon began to work as uſual: the queen laid eggs, which I little ſuſpected to be impregnated, as I thought I had ſeparated all the drones or males, and therefore omitted watching the bees; but at the end of twenty days (the uſual time of their hatching) I found to my ſurprize ſome of the eggs hatched into bees, others withered away, and ſeveral of them covered with honey. I immediately inferred that ſome of the males, having eſcaped my notice, had impregnated only part of the eggs; but, in order to convince myſelf of the truth of my ſuppoſition, I thought it neceſſary to take away all the brood-comb that was in the hive, in order to oblige the bees to provide a freſh quantity, being fully determined to watch narrowly their motions after new eggs ſhould be depoſited in the cells. This was done accordingly, and at laſt the myſtery was unravelled. On the ſecond day after the eggs were placed in the cells, I perceived the ſame operation which I have related in a former experiment; I mean, the bees hung down in the form of a curtain, while others thruſt the poſterior part of their body into the cells: I then introduced my hand into the hive, broke off a piece of the comb containing two of thoſe inſects, and kept them for examination. I found in neither of them any ſting, (a circumſtance peculiar to drones only); and upon diſſection, by by the help a Dolland's microſcope, diſcovered in them the four cylindrical bodies, which contain the glutinous liquor of a whitiſh colour, obſerved by Maraldi in the large drones.

Having till then never obſerved any difference in the ſize of drones, I immediately peruſed the memoirs on bees, publiſhed by Meſſ. Maraldi and Reaumur, and found that they had remarked it frequently. I have inſerted, in a preceding page, the ſubſtance of their obſervations on that head, as taken from their writings. The reaſon of that difference muſt, I doubt, be placed amongſt other *arcana* of nature. I found myſelf, therefore, under a neceſſity, in my next experiments, to be more particular in deſtroying the males, even thoſe which might be ſuſpected to be ſuch.

I once more immerſed all the ſame bees in water; and, when they appeared to be in a ſenſeleſs ſtate, I gently preſſed every one of them between my fingers, in order to diſtinguiſh thoſe armed with ſtings from thoſe which had none, which laſt I might ſuſpect to be males. Of theſe I found fifty-ſeven, exactly of the ſize of common bees, yielding a little whitiſh liquor on being preſſed between the fingers. I killed every one, and replaced the ſwarm in a glaſs-hive, where they immediately applied again to the work of making cells; and on the fourth or fifth day, very early in the morning, I had the pleaſure to ſee the queen-bee depoſiting her eggs in thoſe cells, which ſhe did by placing the poſterior part of her body in each of them. I continued on the watch moſt part of the

enſuing

ensuing days, but could discover nothing of what I had seen before.

The eggs, after the fourth day, instead of changing in the manner of catterpillars, were found in the same state they were in the first day, except that some of them were covered with honey. But a very singular event happened the next day about noon: all the bees left their own hive, and were seen attempting to get into a neighbouring common hive, on the stool of which I found their queen dead, having, no doubt, been slain in the engagement. The manner in which I account for this event is as follows: the great desire of perpetuating their species, which is most observable in these insects, and to which end the concurrence of the males seems so absolutely necessary, made them desert their own habitation, where no males were left, in order to fix their residence in a new one, in which, there being a good stock of males, they might the better accomplish their purpose. If this does not yet establish the reader's faith of the necessity of the males bearing a share in the fecundation of the *ova*, the next experiment cannot, I presume, fail to convince him.

I took the brood comb which, as I observed before, had not been impregnated; I divided it into two parts; one I placed under a glass-bell, N° 1. with honey-comb for the bee's food; I took care to leave a queen, but no drones, among the common bees I confined in it. The other piece of brood comb I placed under another glass-bell, N° 2. with a few drones, a queen, and a number of common bees, proportioned to the size of the glass;

glaſs; the reſt I diſpoſed of as before. The reſult was, that in the glaſs, N° 1. no impregnation happened: the eggs remained in the ſame ſtate they were in when put into the glaſs; and, upon giving the bees their liberty on the ſeventh day, they all flew away, as was found to be the caſe in the former experiment: whereas, in the glaſs, N° 2. I ſaw, the very day after the bees had been put under it, the impregnation of the eggs by the drones in every cell containing eggs; the bees did not leave their hive on receiving their liberty; and, in the courſe of twenty days, every egg underwent all the abovementioned neceſſary changes, and formed a pretty numerous young colony, in which I was not a little ſtartled to find two queens.

Fully ſatisfied concerning the impregnation of the eggs by the males, I deſiſted for the preſent from any further experiments on that head, being exceedingly anxious to endeavour to account for the preſence of this new queen.

I conjectured, that either two queens, inſtead of one, muſt have been left among the bees I had placed under that glaſs; or elſe, that the bees could, by ſome particular means of their own, transform a common ſubject into a queen.

In order to put this to the teſt, I repeated the experiment with ſome variation. I got four glaſs-hives blown flat, which I thought preferable to the bell-ſhaped ones I had uſed before, as I could with thoſe better examine what was going forward. I took a large brood-comb from an old hive, and, after having divided it into ſeveral pieces, I put ſome

ſome of them, containing eggs, worms, and nymphs, with food, viz. honey, &c. under each of the glaſſes; and confined within each a ſufficient number of common bees, among which I left ſome drones, but took care that there ſhould be no queen.

The bees finding themſelves without a queen, made a ſtrange buzzing noiſe, which laſted near two days; at the end of which they ſettled and betook themſelves to work: on the fourth day I perceived in each hive the beginning of a royal cell, *a certain indication that one of the encloſed worms would ſoon be converted into a queen*. The conſtruction of the royal cell being nearly accompliſhed, I ventured to leave an opening for the bees to get out, and found that they returned as regularly as they do in common hives, and ſhewed no inclination to deſert their habitation. But, to be brief, at the end of twenty days I obſerved four young queens among the new progeny.

On relating the reſult of theſe experiments to a member of this univerſity, well converſant in the natural hiſtory of bees, he deemed it neceſſary that they ſhould be repeated, in order the better to eſtabliſh the truth of a fact, ſeemingly ſo improbable, that the eggs, deſtined by Nature to produce neutral or common bees, ſhould be transformed into females or queens. He ſtarted an objection to me, which, by the publication of Mr. Schirach appearing a little time after, ſeems to have been pointed out to that author alſo by Mr. Withelmi, his brother-in-law, namely, that the queen-bee of a hive, beſides the eggs which ſhe depoſits in the royal cells, might

 alſo

alſo have laid royal or female eggs, either in the common cells, or indiſcriminately throughout the different parts of the hive. He further ſuppoſed, that in the pieces of brood-comb, which had been ſucceſsfully employed in the laſt experiments for the production of a queen, it had conſtantly happened, that one or more of theſe royal eggs, or rather the worms proceeding from them, had been contained.

But the force of his objection was removed ſoon after, by the ſame ſucceſs having attended a number of other experiments which I ſince made, an account of which would take up too much room here; and this Gentleman, together with Mr. Schirach's brother-in-law, was at laſt brought to admit, that the working-bees are inveſted with a power of raiſing a common ſubject to the throne, when the community ſtands in need of a queen; and that accordingly every worm of the hive is capable, under certain circumſtances, of becoming the mother of a generation: that it owes its metamorphoſis into a queen, partly to the extraordinary ſize of the cell, and its particular poſition in it; but principally to a certain nouriſhment appropriated to the occaſion, and carefully adminiſtered to it by the working-bees while it is in the worm-ſtate, by which, and poſſibly other means as yet unknown, the developement and expanſion of the germ of the female organs, previouſly exiſting in the embryos, is effected, and thoſe differences in its form and ſize are produced, which afterwards ſo remarkably diſtinguiſh the queen from the common working-bees. And finally it it appears evident, from the experiments made by Mr. Schirach and myſelf, that the received opinion, that the queen lays a particular kind of eggs, appropriated to the production of other queens, is erroneous. I am not a little flattered with the ſimilarity of my diſcoveries with thoſe of the ingenious German naturaliſt, in proving the ſex of the common bees, although we ſo widely differ in what relates to the uſe of the males, whom, as we have ſeen before, he imagines to be quite uſeleſs. I am alſo not a little pleaſed to find, that our experiments on the production of a queen from a common embryo agree ſo well.

I ſhall now beg leave to point out the advantage that may accrue to the public from theſe obſervations, which is that of forming artificial ſwarms or new colonies; or in other words, of furniſhing the means to bring on a numerous increaſe of thoſe uſeful inſects: an object of ſome importance to this kingdom, as being the only means to prevent the annual exportation of conſiderable ſums in the purchaſe of wax, a great quantity of which is loſt every ſeaſon for want of keeping up a ſufficient ſtock of bees to collect it.

The practice of this new art, Mr. Schirach tells us, has already extended itſelf through Upper Luſatia, the Palatinate, Bohemia, Bavaria, Sileſia, and even in Poland. In ſome of thoſe countries it has excited the attention and patronage of government; and even the empreſs of Ruſſia has thought it of ſuch importance, that ſhe has ſent a perſon to Klein Bautzen, to be inſtructed in the general prin-

 ciples,

ciples, and learn all the *minutiæ* of this new art.

The narrow limits of this paper do not permit me here to give an account of Mr. Schirach's ingenious obſervations. I beg leave to refer the curious reader to the work itſelf, which, with the reviewers, I wiſh was tranſlated into the Engliſh language, as it contains many particulars highly deſerving the notice of the ſpeculative naturaliſt, as well as of thoſe who cultivate bees either for profit or amuſement.

PART

PART OF A

LETTER

FROM THE

ABBÉ SPALLANZANI,

TO THE

MARQUIS LUCCHESINI,

CHAMBERLAIN TO THE KING OF PRUSSIA.

I WAS ſome time ago favoured, by your means, with a letter from the king. I had before been agreeably ſurprized by the repeated inſtances of eſteem and kindneſs, with which that great monarch has condeſcended to honour me at different times: but I now feel much greater ſatisfaction than ever, on account of his gracious acceptance of my laſt publication (*a*), which I humbly offered him by you. In conſequence of his command you made an extract, which he had the goodneſs

(*a*) His Diſſertations.

 to

to read, and to confider as not unworthy of his royal approbation. Two particulars, you inform me, efpecially drew the attention of this acute philofopher: the firft, my difcoveries refpecting the generation of feveral amphibious animals. By thefe obfervations he thinks, that the doctrine of *Epigenefis* is fully confuted, and that of the pre-exiftence of germs eftablifhed, an opinion to which he is much inclined. The other particular is, the artificial fecundation of various animals. This made the deepeft impreffion on his mind, as I collect from your following expreffions: "But no part of your book excited fuch aftonifhment as that which treats of artificial fecundation. On reading the analyfis of it he thought of a thoufand experiments, worthy of the notice of the naturalift, as to extend the difcovery from particulars to general, and to enlarge, if it be poffible, the kingdom of animated nature by new colonies of various forts of mules." Thefe two views are certainly the moft interefting of all which natural philofophy affords; but I now confider them as more interefting than ever, fince they have appeared fo agreeable in the eyes of this philofophic king. With refpect to the firft, I rejoice exceedingly with the favourers of the pre-exiftence of germs, among whom are included the moft judicious naturalifts of the prefent age, that we are joined with fo wife and great a monarch; and I cannot but be proud of his glorious approbation, fo gracioufly accorded to my difcovery.

With

With refpect to artificial fecundation, the fubject being, I prefume, worthy of your attention, permit me to detain you a little while upon it.—And in the firft place, a few words concerning amphibious animals. Finding that I had reaped in a field which I may term my own, I was tempted to revifit it, by the hope of adding fome new ears to what I have already gathered. After the publication of my work I therefore made frefh experiments, which proved fortunate beyond all belief. Words cannot exprefs the abundance of the produce. I knew, by experience, that thofe topics of natural hiftory, which we fuppofe to be exhaufted by our induftry and patience, when refumed, prefent themfelves under new and unexpected afpects, whence refult either new truths or ufeful confequences: all the works of the fupreme Architect being ftamped with the feal of his infinite perfections, cannot be exhaufted by human induftry. I will not detain you with the particulars of my new obfervations on the artificial fecundation of my amphibious animals, for the abundance of matter is ill adapted to the limits of a letter. I will rather take the liberty of doing it, when I fhall have fufficient leifure to extract from my journals the fum of my obfervations, in order to compofe a fupplement to my publication on that fubject.

Meanwhile, I pafs on to the artificial fecundation of quadrupeds, concerning which I will obferve to you, that I feel the greateft fatisfaction at finding that my fentiments do not differ from thofe of your fovereign. When I fucceeded in the artificial fecunda-

 tion

tion of a bitch, meditating with ſurprize upon my diſcovery, I conceived that it might be an excellent way to procure, if the thing be poſſible, different ſorts of ſtrange mules, an idea in which my illuſtrious friend Mr. Bonnet, to whom I uſually firſt communicate my experiments, concurred. Hence I reſolved to provide myſelf, at my convenience, with a number of female quadrupeds, as cats, bitches, rabbits, and to try to fecundate them with the ſeed of ſome different ſpecies, at the ſeaſon of their amours. I likewiſe communicated this idea to Dr. Roſſi, a celebrated Profeſſor in the Univerſity of Piſa, that he might put it in practice: Dr. Roſſi, as you perhaps know, is the naturaliſt, who laſt year repeated, with ſucceſs, my experiment on the artificial fecundation of a bitch. Towards the middle of laſt November, when I returned to the univerſity of Pavia, I procured two cats, one two years old, which had once brought young, the other eleven months old, and which had never produced. Both had the liberty of my chamber, but they could not get out, nor was any male ever permitted to enter: there was only in the ſame apartment a little ſpaniel three years and a half old, the ſame which had furniſhed the prolific liquor that fecundated the bitch. The older cat was firſt in ſeaſon; this fell out on the 3d of December: inceſſant loud cries, ſufficiently expreſſive of her wants, afforded a clear proof, that ſhe now began to ſeek and to invite the male. Being unable to ſatisfy her deſires by means of a male of her own ſpecies, and being by nature, like all other female

female cats, exceedingly ſalacious, ſhe for the preſent forgot her antipathy to the dog-kind, and did not heſitate to approach the ſpaniel, and to invite him, by ſtroaking his belly, and reiterated careſſes; but he, without either hurting the cat or flying, never conſented to her wiſhes, though he was of a very voluptuous diſpoſition; he would ſmell her, and then turn with indifference another way. The third day after the appearance of theſe ſigns, I tried to fecundate my cat artificially with twenty-two grains of ſeed, furniſhed by the ſame dog. The ſame means and precautions were employed as in the experiment on the bitch. But having obſerved that the females of this ſpecies receive the male many times, I was not ſatified with a ſingle injection, but repeated it thrice more before the cat went off her heat, which happened on the 11th of December; I kept her confined along with the other, as in a former experiment.

You may conceive my anxious expectation of the reſult of this unattempted experiment. Should any one of my injections prove prolific, and ſhould the young partake, both in form and manners, of the female which conceived them, and the male that furniſhed the ſeed, I fancied, that the moſt ſingular mules, and ſuch as had never been before ſeen, would now be produced. With reſpect to manners, two moſt oppoſite natures would be kneaded together and be confounded; the one, that of an animal ſuſceptible of education, full of courage, abilities, and ſentiment, all ardour, all affection, all obedience to his maſter;

maſter; the other, that of an animal in internal qualities, far inferior, by inſtinct intractable, abhorring all ſubjection, faithleſs to its owner, affectionate only through intereſt, and born with an irreconcileable enmity to the former. Nor would the nature of theſe two animals engrafted together, be leſs different in a phyſical point of view, whether we conſider the external configuration, the proportion of the limbs, or the internal organization. But unhappily this was an occurrence not eaſy to be brought about by the experimenter, and in which his labour is not crowned with ſucceſs. The cat, notwithſtanding all my care, was not fecundated. I was not, however, diſcouraged by this failure from repeating my attempt with the ſeed of the ſame dog and the ſame precautions, upon the other cat, which began to be in heat on the 18th of January, and inſtead of four, I injected ſeed ſeven times; that is to ſay, once every day as long as the ſeaſon of her amours laſted. At each injection I did not introduce leſs than eighteen grains of ſeed; but impregnation did not take place; for from the laſt injection to the date of this letter, thirty-two days have elapſed, and there does not appear the leaſt intumeſcence of the cat's belly. This is alſo the caſe with the other, though the experiment was made ſo long before. We know, that theſe animals bring forth in about 55 days, and bitches in ſixty-three at fartheſt.

I would not, notwithſtanding, pronounce the attempt impracticable, for I think that,

to

to warrant ſuch an aſſertion, a greater number of trials is neceſſary. Theſe two experiments, however, may juſtly render us miſtruſtful of any that ſhall be hereafter attempted; I ſhall not be ſurprized if they ſhould prove unſucceſsful, conſidering the widely different nature of theſe animals. But ſhould this really happen, we ought not to be diſcontented, ſince Nature has thus replied to our interrogatories; and her reſponſes, whatever they may be, ſhould be held precious by us, as they ſerve to increaſe the ſtores of uſeful knowledge. Further, the failure of theſe attempts, ought not to prevent us from making others upon animals differing in their nature. It is true, that every kind of ſeed will not fecundate every ſpecies of animal. This liquor, on which depends the perpetuity of the ſpecies, muſt have a certain relation with the embryos to be fecundated; and it is natural to ſuppoſe, that ſuch a relation does not belong to all kinds of ſeed. But it is alſo true, that we can only learn from the effects, that is from experiment, when this relation does ſubſiſt. The very experiments which at firſt ſeemed contrary to the production of ſuch and ſuch mules, when repeated in a better manner, proved favourable to it. Buffon deſtroyed our hopes of procuring mules, by keeping rabbits and hares together, ſeeing that in the experiment he adduces, the one ſpecies never copulated with the other. But this conjunction has been effected by other hands (*a*), and hence *hare-rabbits* have been

(*a*) In thoſe of the Abbé Amoretti.

procured.

procured. A dog and a ſhe-wolf, kept together for a long time by the ſame author, never ſhewed any ſign of mutual attention. But the experiment, when repeated by others, had a very different degree of ſucceſs. The mules that were by theſe means produced propagated their kind (*a*): Buffon failed in the ſame manner with reſpect to dogs and foxes: this experiment has not, as far as I know, been repeated by others. I ſhould think, that in more expert hands, it would have been attended with a more fortunate reſult. But in the race of mules, there is nothing perhaps ſo curious and ſurprizing as the famous Jumart. Three varieties, you know, are enumerated, the offſpring of the bull and mare, the aſs and cow, and the bull and ſhe-aſs. Leger and Shaw admit the exiſtence of all without heſitation; but Buffon, in his hiſtory of animals, reckons them all imaginary. Yet in his ſupplement, he does not abſolutely deny the poſſibility of their exiſtence, though he doubts it much. But in truth, the French Pliny was miſtaken. Mr. Bourgelat, formerly inſpector-general of the *Ecole Veterinaire* at Lyons, in a letter to the illuſtrious Bonnet, expreſly ſays, that he had been in poſſeſſion of ſeveral of theſe Jumarts, and that one was diſſected under his inſpection in the ſchool at Lyons; and he communicates the reſult in his letter to the philoſopher of Geneva (*b*). The authority of this celebrated and ingenuous perſon, merits

(*a*) *Bonnet* Œuvres, T. 3. (*b*) l. c.

the

the utmoſt deference. Aſſuming then the exiſtence of this ſingular ſort of mule, I could wiſh that they ſhould be multiplied much more than they have hitherto been, both becauſe they are well adapted to throw fuller light upon the great function of generation, and becauſe they may poſſibly prove highly advantageous to mankind, as they are ſaid to have been poſſeſſed of extraordinary ſtrength. Natural fecundation, tho' ſtudiouſly attempted, would not very fully accompliſh this object, on account of the indifference, or rather the averſion of quadrupeds of various ſorts to copulate together, eſpecially when they happen to be placed at a great diſtance from each other. Theſe illegitimate marriages take place only, when the aſs or the bull cannot find the means of ſatisfying their deſires upon their own ſpecies, and are moreover uncommonly ardent. Artificial fecundation, properly performed, would be moſt convenient in this caſe. I will add, moſt reſpectable Marquis, that I am well diſpoſed to put it in practice; but my public and private engagements have hitherto prevented me, as alſo the expence, for I will openly acknowledge it, of keeping theſe animals for ſeveral months, to which the narrow income of a philoſpher is not very adequate. Hence I made application to a rich perſon in Lombardy, to aſſiſt me in theſe experiments; but he was inſenſible to the propoſal, a circumſtance which did not ſurprize me, as it certainly will not ſurprize you, who know,

know, that the nobility in many cities of Italy are not very friendly to ſcience and literature. In order to ſatisfy my wiſhes, I ſee no better expedient, you will excuſe my philoſophical freedom, than that of applying to you. The high honour you enjoy, in poſſeſſing a ſtation in the court of one of the greateſt princes upon earth, ſuitable to your eminent virtues and valuable endowments, of a prince, who is at once the delight of his happy kingdom, and the great protector of letters and learned men: your ardour for natural philoſophy, and for whatever has a tendency to ennoble and advance it; your cloſe connection with our famous royal academy of ſciences and polite literature at Berlin, which is ſo deſirous of enlarging the limits of this noble branch of knowledge, by the ſure guidance of accurate experiments: theſe favourable circumſtances afford me hopes, that you will not refuſe to ſecond my wiſhes. I am willing to believe, that you will chooſe the moſt advantageous moment to ſpeak to your ſovereign of this curious project; nor do I deſpair of his encouragement, ſince it may, in ſome ſort, be termed a thought of his own. But enough of this. I proceed to another ſubject.

[*The remaining part of this letter, having no connection with any topic treated in the foregoing Diſſertations, is omitted.*]

SPAL-

SPALLANZANI having adverted, in more Paſſages than one, to the FECUNDATION OF FISHES, Mr. FERRIS was induced to ſend the following OBSERVATION to the Editor of the *Journal de Phyſique*.

AT the age of about fourteen or fifteen, he happened to be on the bank of a river abounding in fiſhes. The ſtream was rapid, but the water was ſo ſhallow and clear, that at about the depth of two feet, he obſerved two ſalmons ſtirring the ſand with their tails, which were turned to each other. They made a hole in the ſhape of a funnel, over which the female placed the extremity of her tail, and diſcharged a quantity of red liquor: her place was then immediately taken by the male; who, in the ſame poſition, emitted a conſiderable jet of white liquor. They then in concert covered the hole with ſand and parted.

This obſervation is ſucceeded by a paper, copied from Mr. Duhamel's work on fiſhes and fiſheries, on the mode of producing ſalmons and trouts, as it is practiſed on the banks of the Weſer. It is not ſurpriſing that an eſſay, written chiefly with œconomical views, ſhould not in every reſpect ſatisfy the curioſity of the philoſopher. A box or caſe of wood is directed to be made with an aperture towards the bottom of two oppoſite ſides. The aperture is to be ſecured with a grate of iron. The bottom is to be covered with ſand and gravel, and a gentle ſtream is

to

to be brought through the box. In November, when the ſalmon repairs to brooks and rivulets, in order to propagate the ſpecies, a female is to be held by the head over a bucket of clean water. If the eggs be quite mature, they will drop out of themſelves; or if they ſhould not, gentle preſſure on the belly will bring them out. The male is to be treated in the ſame manner, and when the ſurface of the water appears white with milt, the operation is finiſhed. The eggs are now to be carried to the box. Sometimes the young are formed in five weeks, and may be ſeen to move in the egg. They may be diſtinguiſhed by their eyes, which are black, while the other parts are yet diaphanous: in eight days afterwards they break through the ſkin or membrane of the egg. This period, however, varies with the temperature of the water, and of the atmoſphere, and is ſometimes prolonged to ten weeks. While the young fry is growing in the egg, a fine pellicle, diſtinct from the external membrane, may be obſerved; to this pellicle the little fiſh is attached; it forms a ſac round it. The ſac fills almoſts the whole capacity of the egg, and ſerves the fetus *for a ſtomach and bowels*. The firſt feeds upon the matter contained in it four or five weeks after it is hatched. During this time the mouth, which is at firſt ſhapeleſs, gradually elongates; afterwards the ſac totally diſappears, and the animal aſſumes its perfect figure.

The ſame method is to be obſerved with reſpect to trouts, but their eggs are not mature till December and January.

The

The author obſerves, that fiſhes do not copulate, and that fecundation is external. He procured from a trout ſome eggs perfectly mature, and took the utmoſt care of them, but he put no milt upon them, and they all ſpoiled without producing a ſingle young fiſh.

Among the trouts produced according to the method above-deſcribed, there appeared many varieties of monſters; the author explains theſe phænomena from Lewenhoeck's hypotheſis concerning the ſpermatic worms.

But the moſt curious experiment in this paper is the following. The author took the mature eggs out of a trout, which had been dead four days, and which was now very putrid and offenſive. He covered them with the milt of a living male, and the produce of fry was as abundant, as if the eggs had been furniſhed by a living female.

He next propoſes to fecundate the eggs of the trout with the milt of the ſalmon, and reciprocally. He pretends, that certain waters, and a particular kind of food, will convert trouts into ſalmon-trouts.

He aſſerts, that the eggs of both theſe ſpecies infallibly periſh, if any impurity adheres to them, or if they lie long upon the ground: hence he deduces the final cauſe of their depoſiting their offspring upon the gravelly bottom of rivulets, in places where the ſtream is continually freeing them from impurities.

AN

ANALYTICAL INDEX

OF THE

CONTENTS

OF THE

DISSERTATIONS.

CHAP. I.

GENERATION OF THE GREEN AQUATIC FROG.

CHAP. II.

GENERATION OF THE TREE-FROG.

XXXIX.

C H A P. III.

GENERATION OF THE TOAD, CALLED BY ROESEL, BUFO TERRESTRIS, DORSO TUBERCULIS EXASPERATO, OCULIS RUBRIS.

C H A P. IV.

GENERATION OF THE FETID TERRESTRIAL TOAD.

LXXIV.

CHAP. V.

GENERATION OF THE WATER-NEWT.

C H A P. VI.

REFLECTIONS.

CVIII.

C H A P. VII.

EXAMINATION OF DR. PIRRI'S OBJECTIONS.

DISSERTATION II.

C H A P. I.

ARTIFICIAL FECUNDATION OF THE TERRESTRIAL FROG, WITH RED EYES AND DORSAL TUBERCLES.

C H A P. II.

ARTIFICIAL FECUNDATION OF THE WATER-NEWT, AND FETID TERRESTRIAL TOAD.

C H A P. III.

ARTIFICIAL FECUNDATION OF THE TREE-FROG, AND THE GREEN AQUATIC FROG.

CHAP. IV.

REFLECTIONS.

CLVIII.

CHAP. V.

WHETHER FECUNDATION IS AN EFFECT OF THE AURA SEMINALIS. WHETHER OTHER LIQUORS ARE CAPABLE OF PRODUCING FECUNDATION. TRIALS TO PROCURE ARTIFICIAL MALES IN TAE AMPHIBIOUS ANIMALS IN QUESTION. ARTIFICIAL FECUNDATION OF THE SILK-WORM. ATTEMPT TO IMPREGNATE A BITCH ARTIFICIALLY.

DISSERTATION III.

CHAP. I.

GENERATION OF THE PLANTS DENOMINATED BY LINNÆUS, SPARTIUM JUNCEUM, VICIA FABA, PISUM SATIVUM, DOLICHOS UNGUICULATUS.

CHAP. II.

GENERATION OF THE RAPHANUS SATIVUS, CICER ARIETINUM, IXIA CHINENSIS, DELPHINIUM CONSOLIDA, CUCURBITA PEPO, CUCUMIS SATIVUS. EXAMINATION OF THE POWDER OF THE STAMINA.

CHAP. III.

GENERATION OF SOME HERMAPHRODITE AND MONŒCOUS PLANTS, ON WHICH THE POLLEN NEVER ACTED.

CHAP. IV.

GENERATION OF CERTAIN PLANTS PRODUCING MALE AND FEMALE INDIVIDUALS, ON WHICH THE FECUNDATING DUST WAS PREVENTED FROM EXERTING ITS INFLUENCE.

XXIV.

C H A P. V.

RECAPITULATION. REFLECTIONS.

THE END.

TRANSLATION

OF THE

LATIN, FRENCH, and ITALIAN PASSAGES in this VOLUME.

Page 9. Neither fecundation or impregnation of the egg, takes place in any living body in Nature without the body of the female.

Page 46. *All things were created good.*

Page 90. The male frog, firmly ſeated on the female, waits for diſcharge of the eggs; he then emits his embryos, ſuch as I obſerved them; they attach themſelves to the eggs, and feed upon them for a few days, till they are capable of taking coarſer food. Theſe embryos retain the figure which they had in the veſicle of the male for about a month; they then change their ſhape like ſilk-worms. Their hind feet are next developed and ſeparated; the hind feet were at firſt united, and formed the tail of the tadpole.

Page 92. l. 15. He advances as ſerious truths, tales equally ridiculous with thoſe told by old women, as they ſpin by the fire-ſide.

Ib. l. 25. I will therefore venture to aſſert, not only that Gautier had made little proficiency in the anatomy of the frog, but that he was ſcarce acquainted with the external form of this animal.

Ib. l. 34. Were it poſſible that Gautier ſhould relate his diſcoveries to Pythagoras, I firmly believe, that he would enjoin him ſilence, not for two or for five years, but for ever.

Page 98. As often as the female diſcharges any eggs, the male bedews them with ſeed, as I have repeatedly obſerved at my houſe, with ſome admiration.

Page 104. I reſign this Dr. Pirri to you. He is in good hands, and you will be ſufficiently able to defend the good cauſe of Nature. It is always a mark of temerity to attack experiments by reaſoning.

Page

Page 105. Laftly, we have a demonftration, which directly proves the exiftence of the young in the female, at leaft in birds. For the inteftine of the young bird is a continuation of the membrane of the yolk, and the internal coat of the inteftine is a continuation of the epidermis, the external of the fkin: in fhort, it is the fame as the membrane of the yolk.

Page 199. Nature, when I have been contemplating her, has often perfuaded me to fuppofe nothing incredible which relates to her.

Page 331. Should the hemp, producing male flowers, be pulled before the feminiferous individuals have opened their piftiliferous flowers, there will either be no crop of feeds, or only a very fmall one.

Page 337. We diftinguifh the obferver from the adherent of fyftem. The latter builds whole fyftems upon a certain number of fpecies: he concludes from particulars to generals, that is, he affigns to all plants the fame properties as are poffeffed by thofe upon which experiments have been made. The obferver rejects all theories, and relies on obfervation and experiment alone. The perfection of botany effentially depends on the knowledge of individuals related to each other, and their characteriftics. This knowledge will be acquired in proportion as the numbers of obfervers, which is very inconfiderable, fhall increafe, and the croud of fyftem-makers fhall be leffened. Syftems of botany become obfolete in time, becaufe they are not chiefly founded on Nature and experiment.

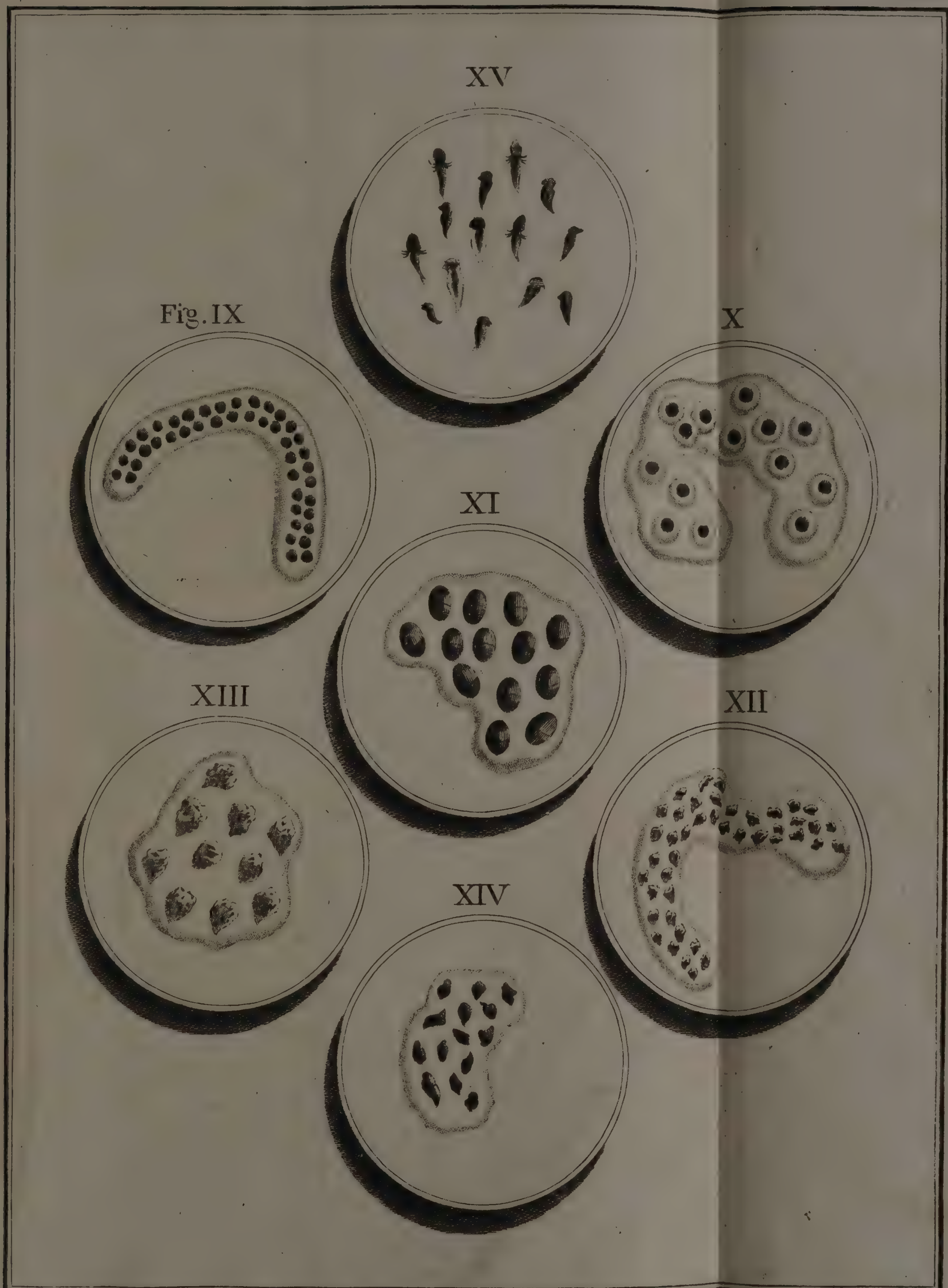
Page 344. He was of opinion, that Natural Hiftory does not confift in a catalogue of animals, plants, and foffils, but in the knowledge of their qualities. He therefore taught their ftructure, analyfis, properties and ufe, without neglecting, however, fuch marks as may enable us to arrange them in an index. Thofe who commit to memory nothing but fuch indexes, or are dexterous in turning them over, in vain perfuade themfelves, that they are in poffeffion of the book of Nature.

E R R A T A.

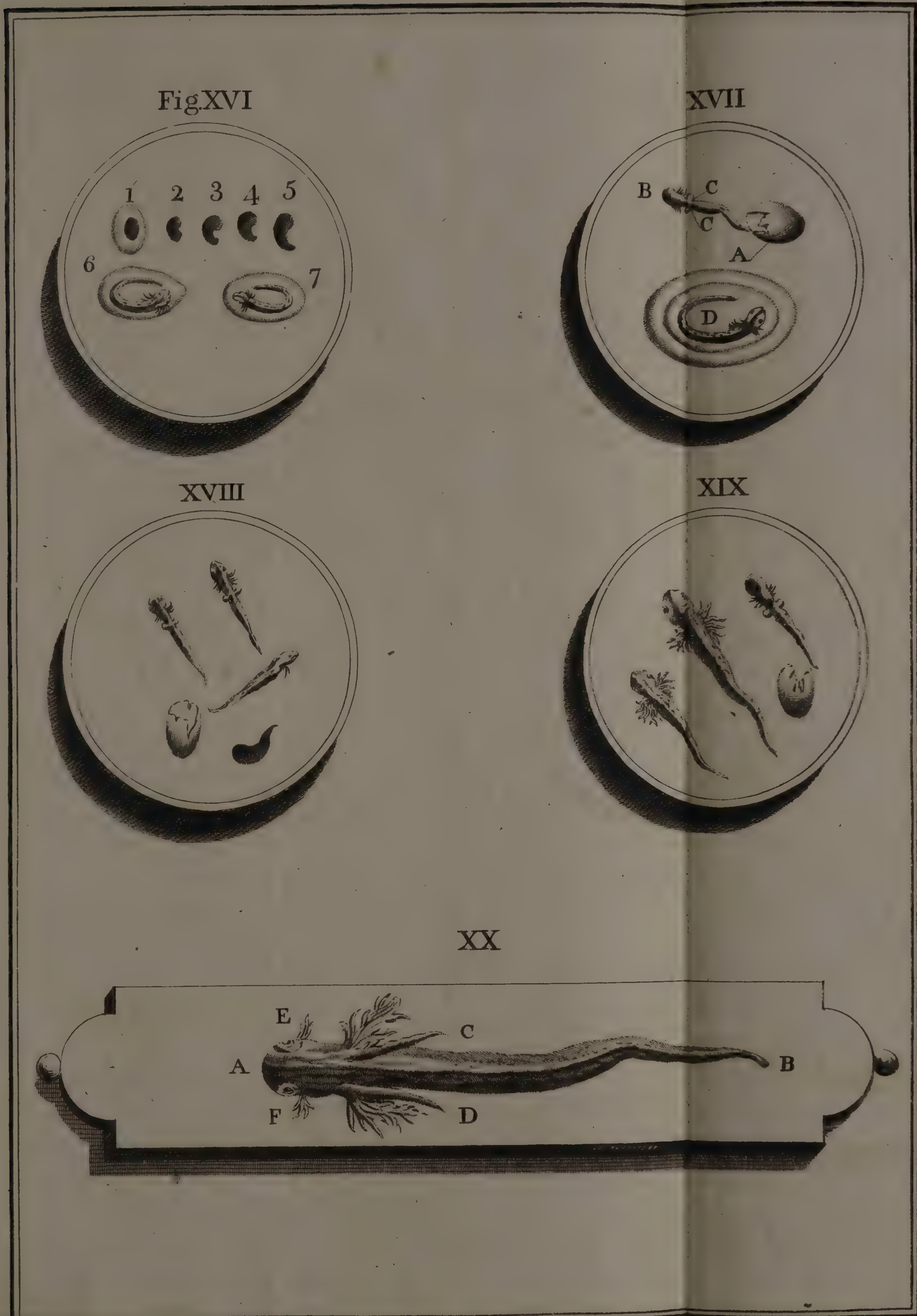
P. 6. for *ix*, read *in*—P. 7. for *fetuſſes*, read *fetuſes* P. 8. for *frog's ſeed*, read *frog's ſpawn*—P. 14. l. 6. and elſewhere, for *amnios* read *amnion*—P. 17. l. 3. for *theſe* read *the*—P. 19 l. 20. for *is* read *be*—P. 80. l. 1. for *they are not*, read *that they are*—P. 97. l. laſt, for *on* read *in*—P. 108. l. 33. after *not* inſert *ſo*—P. 160. for *had no concern*, it ſeems better to read *were not preſent*—P. 184. for *converted*, read *condenſed*—P. 319. l. 25. for *was*, read *were*—P. 322. for *one* read *a*—P. 337. for *immortality*, read *perpetuity*

N. B. In a Paſſage, which the Tranſlator cannot now find, for *impregnated*, read *unimpregnated*. The Context will eaſily enable the Reader to make this Correction in the proper Place.

There are alſo a few Improprieties in the Punctuation, which the Reader is alſo deſired to correct.



Publish'd 1st Jan.y 1784, by I. Murray N.o 32 Fleet street.



Page sc.

Publish'd 1st Jan.y 1784. by I. Murray No. 32 Fleet street.